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PHILOSOPHICAL TRANSACTIONS,
OF THE
ROYAL SOCIETY
OF
LONDON.

VOL. LXXIX. For the Year 1789.

PART I.

BIBLIOTHECA BODLEIANA

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ADVERTISEMENT.

THE Committee appointed by the *Royal Society* to direct the publication of the *Philoſophical Tranſactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations which have been made in ſeveral former *Tranſactions*, that the printing of them was always, from time to time, the ſingle act of the reſpective Secretaries, till the Forty-ſeventh Volume: the Society, as a Body, never intereſting themſelves any further in their publication, than by occaſionally recommending the revival of them to ſome of their Secretaries, when, from the particular circumſtances of their affairs, the *Tranſactions* had happened for any length of time to be intermitted. And this ſeems principally to have been done with a view to ſatisfy the Public, that their uſual meetings were then continued, for the improvement of knowledge, and benefit of mankind, the great ends of their firſt inſtitution by the Royal Charters, and which they have ever ſince ſteadily purſued.

But the Society being of late years greatly inlarged, and their communications more numerous, it was thought adviſable, that a Committee of their members ſhould be appointed to reconſider the papers read before them, and ſelect out of them ſuch as they ſhould judge moſt proper for publication in the future *Tranſactions*; which was accordingly done upon the 26th of March 1752. And the grounds of their choice are, and will continue to be, the importance and ſingularity of the ſubjects, or the advantageous manner of treating them; without pretending to anſwer for the certainty of the facts, or propriety of the reaſonings, contained in the ſeveral papers ſo publiſhed, which muſt ſtill reſt on the credit or judgment of their reſpective authors.

It

It is likewiſe neceſſary on this occaſion to remark, that it is an eſtabliſhed rule of the Society, to which they will always adhere, never to give their opinion, as a Body, upon any ſubject, either of Nature or Art, that comes before them. And therefore the thanks, which are frequently propoſed from the Chair, to be given to the authors of ſuch papers as are read at their accuſtomed meetings, or to the perſons through whoſe hands they receive them, are to be conſidered in no other light than as a matter of civility, in return for the reſpect ſhewn to the Society by thoſe communications. The like alſo is to be ſaid with regard to the ſeveral projects, inventions, and curioſities of various kinds, which are often exhibited to the Society; the authors whereof, or thoſe who exhibit them, frequently take the liberty to report, and even to certify in the public news-papers, that they have met with the higheſt applauſe and approbation. And therefore it is hoped, that no regard will hereafter be paid to ſuch reports, and public notices; which in ſome inſtances have been too lightly credited, to the diſhonour of the Society.

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THE President and Council of the Royal Society adjudged, for the Year 1788, the Medal on Sir Godfrey Copley's Donation, to Charles Blagden, M. D. Sec. R. S. for his Two Papers on Congelation, printed in the laſt Volume of the Philoſophical Tranſactions.

ERRATUM.

Page 37, line 15. *for* $\frac{175}{5}$ *read* $\frac{175}{7}$

PHILOSOPHICAL TRANSACTIONS.

I. *Deſcription of an Improvement in the Application of the* Quadrant of Altitude *to a celeſtial Globe, for the Reſolution of Problems dependant on* Azimuth and Altitude. *By Mr.* John Smeaton, *F. R. S.*; *communicated by Mr.* William Wales, *F. R. S.*

Read November 20, 1788.

PERHAPS there are few inſtruments that better fulfil their deſign in general, or more naturally repreſent the movements they are intended to explain and illuſtrate, than the terreſtrial and celeſtial globe, which are alſo applied to reſolve ſome of the problems of the ſphere, which they moſt readily do. I believe, however, that whoever applies to them for the laſt mentioned purpoſe, will find them more defective in ſome reſpects than they are in others.

The difficulty that has occurred in fixing a ſemicircle, ſo as to have a center in the *zenith* and *nadir* points of the globe, at the ſame time that the meridian is left at liberty to raiſe the pole to its deſired elevation, I ſuppoſe, has induced the globe-makers to be contented with the *ſtrip* of thin flexible braſs, called the *quadrant of altitude*; and it is well known how imperfectly it performs its office.

The improvement I have attempted, is in the application of a *quadrant of altitude*, of a more ſolid conſtruction; which being affixed to a braſs ſocket of ſome length, and this ground, and made to turn upon an upright ſteel ſpindle, fixed in the zenith, ſteadily directs the *quadrant*, or rather *arc*, of *altitude* to its true *azimuth*, without being at liberty to deviate from a vertical circle to the right hand or left: by which means the azimuth and altitude are given with the ſame exactneſs as the meaſure of any other of the great circles.

With reſpect to the horary circle, as the common application ſeems very convenient on account of the ready adjuſtment of its index to anſwer the culmination of any of the heavenly bodies; and as I find that a circle of four inches diameter is capable of an actual and very diſtinguiſhable diviſion into 720 parts, anſwerable to two minutes of time each, which may ſerve a globe of the largeſt ſize; it ſeems that it ſhould rather be *improved* than omitted; and, if inſtead of a *pointer*, an index *ſtroke* is uſed in the ſame plane with that of the diviſions, the ſingle minutes, and even half minutes, may be readily diſtinguiſhed.

This globe, though mounted merely as a model for experiment, and only nine inches in diameter, appears capable of bringing out the ſolution to a quarter of a degree; which, I apprehend, may be eſteemed ſufficient not only as a check upon numerical computation, but to come near enough to find ſtars in the day-time in the field of teleſcopes, which, having no equatorial motion, are only capable of direction in altitude and azimuth; but from globes of a larger ſize, we may expect to come proportionably nearer.

Explanation of the figures, Plate I.

The figures 1. and 2. being different views of the ſame things, AB repreſents a line, in common to both, in the ſurface of the horizon, which here is of braſs.

CD, CD, are vertical lines, ſuppoſed to paſs through the center of the globe in each figure; and

EFG, EFG, are portions of great circles of the globe.

Fig. 1. ſuppoſes the ſpectator looking at the apparatus of the globe from the ſouth point of the horizon; therefore the circular arch EFG, in this poſition, will be a part of the *prime vertical*, and the ſmall paralellogram HI is ſuppoſed to be a *ſection* of the braſs meridian, according to that vertical plane.

Fig. 2. is a view of the ſame parts, the ſpectator being ſuppoſed to look at them from the weſt point of the horizon; and in this poſition HI is ſuppoſed to be a *portion* of the *braſs meridian.* This being fixed in mind, in what follows the ſame letters denote the ſame parts in both figures.—KLM denotes a piece of braſs, or braſs carriage, made to fit upon the vertical part of the meridian, and capable of ſliding 5° on each ſide of that point, ſo as to adjuſt to it, and to fix faſt there, by means of the finger ſcrew N*. This piece of braſs carries the *ſteel ſpindle* PQ, which is firmly ſocketed into it at K, according to the dotted lines *o r*, *o r*. The axis of this ſpindle is therefore capable of being ſet upright upon the *zenith point*, and to maintain that poſition with a ſufficient degree of firmneſs.—R*q*, R*q*, repreſents the ſection of a braſs ſocket made to fit the ſpindle, and turn round freely upon it; and when home to the ſhoulder at *o o*, to turn without ſhake; the ſocket and ſpindle being a ſmall matter taper, and *ground* together. On one ſide of the ſocket is firmly fixed the arm ST, by ſcrews or ſolder.—UW is an arch of 80 degrees, ſerving inſtead of the quadrant of altitude, and of the ſame *ſubſtance* as the meridian. This is firmly ſcrewed to the arm, and adjuſted by conſtruction; ſo that when the *ſpindle* is vertical, the face of this arch ſhall make part of a vertical circle.—This arch being a portion of a circle, of the ſame diameter as the braſs meridian, when its point *zero* at W reſts upon the braſs horizon, its inſide ſurface is made to agree with that of the horizon by means of a ſmall thin *nib* of braſs; that being attached to the inſide of the bottom of the quadrant of altitude at W, and projecting a little below it, gently bears againſt the inſide of the horizon, in ſubſtance occupying about half the *clearance* between the body of the globe and its ſurrounding horizon: this *nib*, ſeen edgeways, is ſhewn at the letter X. By this means the altitude of the object is ſhewn upon the working face of the quadrant, and the quadrant's bottom ſhews the azimuth upon the horizon; at the ſame time the globe is free to revolve upon its axis, clear of all the circles.

---

* The holes repreſented in the portion of the braſs meridian (HI, fig. 2.) are ſcrew holes at five degrees diſtance, in this quarter of the circle, into any of which the finger ſcrew N is to be put as occaſion may require; the *ſlit* allowing ſufficiently for adjuſtment.

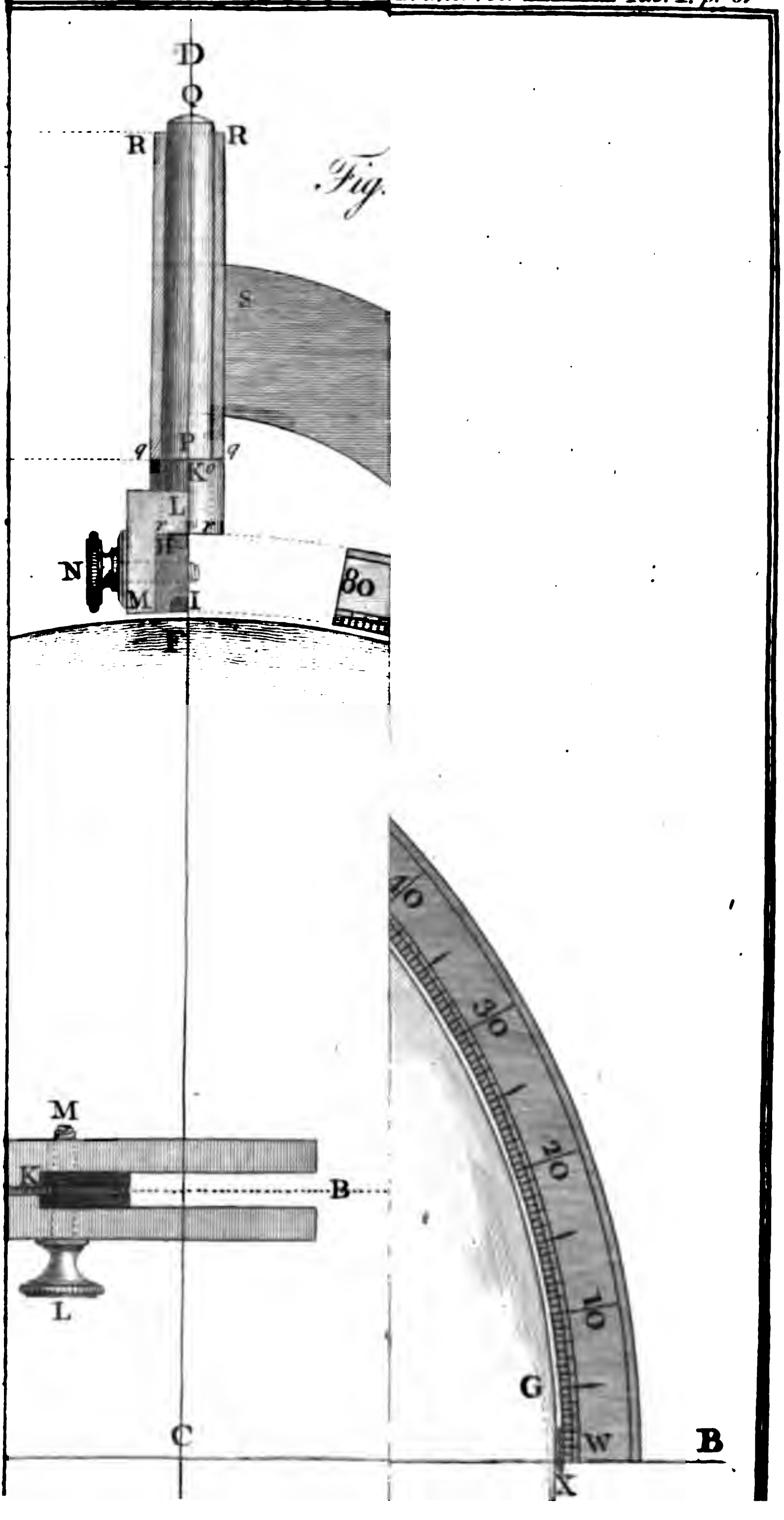
The quadrant might be made complete to 90°; but as in theſe middle latitudes there is very little buſineſs for azimuths when the altitudes are above 80°, and as I judged it eligible, that the quadrant ſhould be made capable of working on both ſides the meridian; *that* would be prevented by the neceſſary thickneſs that the circles require to give them ſolidity, in contradiſtinction to *mathematical planes*; unleſs a part of a quadrant was cut out next the vertex to give them clearance: by this means the arch being lifted up from the ſpindle, and put on the other ſide of the braſs meridian for the afternoon, it will then come within 10° or 15° of the meridian; and if the uſe of this ſpace ſhould be wanted, it can be ſupplied by reverſing the ſimilar operation for the morning; and the back ſide of the upper end of the quadrant at U being champered, or *bevilled* off, this will admit it to come as near to the meridian as I have mentioned.

The ſteel ſpindle is eaſily adjuſted to the *zenith*; for the globe being rectified to its *latitude*, ſet the braſs carriage at liberty, bring the quadrant and meridian together, face to face, and ſlide the carriage, till the lower extremity of the quadrant *buts* upon the horizon, and there ſcrew it faſt.

It is, however, to be noted, that I have found ſomething neceſſary by way of *holdfaſt*, to prevent the braſs meridian from ſhifting its latitude, and that without confining it in any other reſpect.—What I have found to anſwer this purpoſe is repreſented, fig. 3. The crutch-like piece of wood ABC is ſhewn as ſeen looking right down upon it. The circle DE is the horizontal ſection of the ſouth pillar of the globe. The ſtrong wire pin FG, that goes through the two arms of the crutch and pillar, ſerves as an axis upon which its other extremity at B is at free liberty to lift up and down, but without ſhake upon the pin; and the whole being ſplit with a fine ſaw, from B to H, the notch BK lays hold of the under ſide of the braſs meridian, and by tightening the finger ſcrew LM, it firmly clips it, and retains it in any given poſition. And that it may be under no confinement *croſs-ways*, the hole in the pillar is opened on both ſides, as ſhewn in the ſection, to give it liberty of accommodation; the pin being faſt in the two ends of the crutch, and turning gently in the pillar; the whole being ſlender and compliant, except in point of length.

N. B. Thoſe that would uſe the globe to the beſt advantage to ſolve problems, ſhould be careful to get a *juſt* declination, as alſo a *diſtinct* point to mark it; and as the circles and diviſions upon the ſurface of the globe itſelf, are not always ſufficiently to be depended on for this purpoſe, I have found the following expedient fully to anſwer. Chuſe any plain white part of the globe's ſurface, anſwerable to the declination given, and with the point of a needle or protracting pin, by the help of the diviſions of the braſs meridian, mark a fine point upon the blank ſurface of the globe, and upon this point make a dot with ink, with the ſmall point of a pen, which rub off with the finger, and it will leave a fine black ſpeck behind. This dot being brought to the meridian, rectify the horary index to it, and it will accurately repreſent the center of the celeſtial body whoſe inveſtigation is wanted.

Trans. Vol. LXXIX. *Tab.* I. *p.* 6.



II. *Objections to the Experiments and Obſervations relating to the Principle of Acidity, the Compoſition of Water, and Phlogiſton, conſidered; with farther Experiments and Obſervations on the ſame Subject. By the Rev.* Joſeph Prieſtley, *LL.D. F. R. S.*

Read November 27, 1788.

HAVING never failed, when the experiments were conducted with due attention, to procure ſome *acid* whenever I decompoſed dephlogiſticated and inflammable air in cloſe veſſels, I concluded that an acid was the neceſſary reſult of the union of thoſe two kinds of air, and not water only; which is an hypotheſis that has been maintained by Mr. LAVOISIER and others, and which has been made the baſis of an intirely new ſyſtem of chemiſtry, to which a new ſyſtem of terms and characters has been adapted. The *facts* that I alleged were not diſputed; but to my *concluſion* it was objected, that the acid I procured might come from the phlogiſticated air, which in one of my proceſſes could not be excluded; and that it was reaſonable to conclude that this was the caſe, becauſe Mr. CAVENDISH had procured the ſame acid, *viz.* the nitrous, by decompoſing dephlogiſticated and phlogiſticated air with the electric ſpark. In other caſes it has been ſaid, that the *fixed air* I procured came from the *plumbago* in the iron from which my inflammable air had been extracted.

With reſpect to the former of theſe objections I would obſerve, that my proceſs is very different from that of Mr.

CAVENDISH; his decompoſition being a very ſlow one by electricity, and mine a very rapid one by *ſimple ignition*, a proceſs by which phlogiſticated air, as I found by actual trial, was not at all affected; the dephlogiſticated and inflammable airs uniting, and leaving the phlogiſticated air (as they probably would any other kind of air with which they might have been mixed) juſt as it was.

I would alſo obſerve, that there is no contradiction whatever between Mr. CAVENDISH's experiment and mine, ſince phlogiſticated air may contain phlogiſton, and by means of electricity this principle may be evolved, and unite with the dephlogiſticated air (or with the acid principle contained in it) as in the proceſs of ſimple ignition the ſame principle is evolved from inflammable air, in order to form the ſame union; in conſequence of which, the water, which was a neceſſary ingredient in the compoſition of both the kinds of air, is precipitated. That in other circumſtances than thoſe in which I made the experiments, the acid wholly eſcaped, and nothing but water was found, may be eaſily accounted for, from the ſmall quantity of the acid principle in proportion to the water, and the extreme volatility of it, owing, I preſume, to its high phlogiſtication when formed in this manner.

In order to aſcertain the effect of the preſence of phlogiſticated air in this proceſs, I now not only repeated the experiment of mixing a given quantity of phlogiſticated air with the two other kinds of air, and found, as before, that it was not affected by the operation; but I made the experiment with atmoſpheric air, inſtead of dephlogiſticated. Since the air of the atmoſphere contains a greater proportion of phlogiſticated air, it might be expected that, if the acid I got before came from the ſmall quantity of phlogiſticated air which I could not poſſibly exclude, I ſhould certainly get more acid, when, inſtead of endeavouring to exclude it, I purpoſely introduced a greater quantity. But the conſequence was the production of much leſs acid than before, the liquor I procured being ſometimes not to be diſtinguiſhed from pure water, except by the greateſt attention poſſible: for though the decompoſition was made in the ſame copper veſſel which I uſed in the former experiments, there was now no ſenſible tinge of green colour in it.

When I repeated this experiment in a glaſs veſſel, I perceived, as I imagined, the reaſon of the ſmall produce of acid in theſe new circumſtances: for the veſſel was filled with a vapour which was not ſoon condenſed, and being diffuſed through the phlogiſticated air (which is not affected by the proceſs) is drawn away along with it, when the exhauſting of the tube is repeated; whereas, when there is little or no air in the veſſel beſides the two kinds which unite with each other, and are decompoſed, the acid vapour, having nothing to attach itſelf to and ſupport it (by being entangled with it) much ſooner attacks the copper, making the deep green liquor which I have deſcribed. Sometimes, however, I have procured a liquor which was ſenſibly green by the decompoſition of atmoſpheric and inflammable air, but by no means of ſo deep a colour, or ſo ſenſibly acid, as when the dephlogiſticated air is uſed.

The extreme volatility of the acid thus formed (and which accounts for the eſcape of ſome part of it in all theſe proceſſes) is apparent from this circumſtance, that if the exploſions be made in quick ſucceſſion (the tube being exhauſted immediately after each of them, and filled again as ſoon as poſſible) no liquor at all will be collected, the whole of the acid vapour, together with the water with which it was combined, being drawn off uncondenſed in every proceſs. I once made twenty ſucceſſive exploſions of this kind, in a copper tube, out of which I found that I drew 37 ounce meaſures of air by the action of the pump, and found not a ſingle drop of liquid, though near an hour was employed in the whole proceſs, and the veſſel was never made more than a little warmer than my hand. This was a degree of heat by no means ſufficient to keep the whole of any quantity of water in a ſtate of vapour; and is a circumſtance that of itſelf ſufficiently proves, that the vapour did not conſiſt of water only.

Indeed, I think it impoſſible for any one to *ſee* this vapour in a tall glaſs veſſel, and eſpecially to obſerve how it falls from one end of it to the other, and the time that is required to its wholly diſappearing, without being ſatisfied that it conſiſts of ſomething elſe than mere water, the vapour of which would be more equally diffuſed. If the appearance to the eye ſhould fail to convince any perſon of this, the ſenſe of *ſmell* would do it: for even in a glaſs veſſel it is very offenſive, though it might not be pronounced to be *acid.* I conjecture, however, that this, and every other ſpecies of *ſmell*, is produced by ſome modification of the acid or alkaline principle. Some may be diſpoſed to aſcribe this ſmell to the *iron* from which the inflammable air was produced; but the ſmell is the ſame, or nearly ſo, when the air is from tin, and would probably be the ſame if it were from any other ſubſtance.

Beſides uſing atmoſpheric air, which contains a greater proportion of phlogiſticated air, I have ſometimes uſed dephlogiſticated air which was not very pure; and in this caſe I have always obſerved, that the liquor I procured had leſs colour, and was leſs ſenſibly acid.

Theſe obſervations might, I ſhould think, ſatisfy any reaſonable perſon, that the acid liquor which I procured by the exploſion of dephlogiſticated and inflammable air in cloſe veſſels did not come from the phlogiſticated air which could not be excluded, whether it was that which remained in the veſſel after exhauſting it by the air pump, or that with which the dephlogiſticated air was more or leſs contaminated.

But beſides theſe experiments, in which I procured the green acid liquor by the exploſion of dephlogiſticated and inflammable air in cloſe veſſels, I made another, to which I thought the ſame objection could not have been made, becauſe no air pump was uſed in it, and nothing but the pureſt dephlogiſticated air was employed, being ſeparated in the proceſs from *precipitate per ſe* in contact with the pureſt inflammable air in a glaſs veſſel which had been previouſly filled with mercury. Accordingly, the only objection made to *this* experiment was, that the preparation I made uſe of might be impure, containing ſomething which might yield phlogiſticated air. This appeared to me highly improbable, as the precipitate had been made by M. Cadet, and for the purpoſe of philoſophical experiments. Beſides, if the heat of a burning lens ſhould diſlodge phlogiſticated air from any unperceived impurity in this preparation, mere *heat* will not decompoſe this air. Let any perſon try the effect of a lens on ſuch air, or any ſubſtance containing it, and produce an acid if he can.

M. Berthollet, however, thinking that this might be the caſe, deſired that I would ſend him a ſpecimen of my precipitate *per ſe*. Accordingly, I ſent him all that remained of it; and, in return, he ſent me a quantity on the goodneſs of which I might depend. With this preparation I repeated my former experiment; and, by giving more attention to the proceſs, found it to be far more deciſively concluſive in favour of my opinion than I had imagined. In the former experiment I had attended only to the drop of *water* which was found in the veſſel in which the proceſs was made; and finding that it turned the juice of turnſole red, I concluded, that it contained nitrous acid: but I now examined the *air* that remained in the veſſel, and found that a conſiderable proportion of it was fixed air; ſo that I am now ſatisfied *this* was the acid with which it was impregnated, and not the *nitrous*. Still, however, ſome acid is the conſtant reſult of the union of the two kinds of air, and not water only. A quantity of the ſame precipitate *per ſe* yielded no fixed air by heat.

Comparing this experiment with that in which iron is ignited in dephlogiſticated air, this general concluſion may be drawn, *viz.* that when either inflammable or dephlogiſticated air is extracted from any ſubſtance in contact with the other kind of air, ſo that one of them is made to unite with the other in what may be called its *naſcent ſtate*, the reſult will be *fixed* air; but that if both of them be completely formed before their union, the reſult will be *nitrous acid.*

It has been ſaid, that the fixed air produced in both theſe experiments may come from the *plumbago* in the iron from which the inflammable air is obtained. But ſince we aſcertain the quantity of plumbago contained in iron by what remains after its ſolution in acids, it is in the higheſt degree improbable, that whatever plumbago there may be in iron, any part of it ſhould enter into the inflammable air procured from it. Beſides, according to the antiphlogiſtic hypotheſis, all inflammable air comes from water only.

As it cannot be ſaid, that any real fixed air is found in inflammable air from iron (ſince it is not diſcoverable by lime-water)

water) it muſt be ſuppoſed, that the elements, or component parts of fixed air are in it; but one of theſe elements is pure air, and the mixture of nitrous air ſhews, that it contains no ſuch thing, though, according to M. LAVOISIER, fixed air contains 72 parts in 100 of pure air.

However, being apprized of this objection to inflammable air from iron, I made uſe of inflammable air from *tin*, and I had the ſame reſult as with that from iron. I alſo calculated the weight of the fixed air which I got in the proceſs, and comparing it with the plumbago which the iron neceſſary to make the inflammable could have contained, I found, that, in all the caſes, it far exceeded the weight of the plumbago; ſo that it was abſolutely impoſſible, that the fixed air which I found ſhould have had this origin. For the greater ſatisfaction, I ſhall recite the particulars of a few experiments of this kind.

In ten ounce meaſures of inflammable air from malleable iron I revived *red precipitate* till there remained only 1.1 oz. meaſure of air, and of this 0.07 oz. m. was fixed air, being completely abſorbed by water. The weight of this air would be 0.063 gr. But, ſince 960 grains of iron will yield 1054 oz. meaſures of inflammable air, the iron employed in procuring all the inflammable air that was uſed in this experiment, *viz.* 8.9 oz. meaſures (without allowing for any that went to the revivification of the mercury) would be 8.1 grains; and ſince M. BERGMAN ſuppoſes, that 100 grains of iron contains 0.12 gr. of plumbago, the quantity of it in this iron would only be 0.01008 gr. which is not quite a ſixth part of the weight of the fixed air.

With the *precipitate per ſe*, ſent me by M. BERTHOLLET, I revived mercury till 8½ oz. m. of inflammable air was reduced to 2½ oz. m., and of this 0.04 oz. m. at leaſt was fixed air.

This is not quite ſo much in proportion as in the preceding experiment, but abundantly more than the weight of the plumbago.

In 8 oz. m. of inflammable air I revived *minium* (which I found to have exactly the ſame effect in this proceſs as red precipitate, or precipitate *per ſe*), till it was reduced to 1.2 oz. m.; and of this 0.028 oz. m. was fixed air, which would exceed the weight of the plumbago more than three times. In reviving lead from maſſicot (which I prepared by expelling the pure air from minium) I had no fixed air in the reſiduum.

In 7 oz. m. of inflammable air from tin by ſpirit of ſalt, I revived red precipitate till it was reduced to 1.1 oz. m.; and in this the fixed air was ſomething more than in proportion to that in the laſt experiment.

In my laſt volume of Experiments, p. 30. I mentioned ſome inſtances of the revival of red precipitate in inflammable air, without finding any fixed air, though in one I perceived a ſlight appearance of it. To this I can only ſay, that I now always find it, and have, in the preceding caſes, meaſured the quantity of it; ſo that, though I did not find any before, I muſt preſume that I did not uſe the ſame precautions that I did at this time: and it is poſſible, that I might not attend to the effect of admitting a large quantity of water to a ſmall quantity of fixed air, which would preſently abſorb the greateſt part of it. I alſo think I recollect, that I then continued the proceſs as far as I poſſibly could, and conſequently left very little air in the veſſel; whereas I now purpoſely left a good deal, that the admiſſion of water might have leſs effect on the fixed air diffuſed through it.

This alſo may be ſaid in favour of the greater accuracy of my preſent experiments, that they intirely remove a very great difficulty, which I acknowledged, p. 128. in finding different reſults from ſeemingly ſimilar circumſtances; whereas I now find that both the circumſtances and the reſults are different. Beſides, the *poſitive* evidence of actually finding a ſubſtance is always more concluſive than the *negative* one, of not finding it.

I do not know that any objection can be made to the inflammable air from *tin*, as this metal has not been proved to contain plumbago. I wiſhed, however, to repeat this experiment with inflammable air from *ſulphur*. But though, when ſteam is ſent over melted ſulphur, a ſmall quantity of inflammable air is procured, as I obſerved in my laſt volume of experiments; yet, as ſulphur cannot part with much phlogiſton, except in proportion as it imbibes pure air, to form oil of vitriol, I could not in this manner eaſily procure enough for my purpoſe.

In order to ſupply the ſulphur with pure air, I mixed with it a quantity of *turbith mineral*; but this made it yield vitriolic acid air, though in great abundance, there not being, I imagine, *water* enough to form inflammable air: for when iron is diſſolved in concentrated acid of vitriol, vitriolic acid air is produced; but in diluted vitriolic acid, the produce is inflammable air. With a view to ſupply theſe materials with water, I ſent ſteam over them; but it did not combine with the air, which was ſtill only vitriolic acid air.

Since, however, vitriolic acid air unqueſtionably contains the ſame principle which forms the inflammability of inflammable air, this experiment proves, that ſulphur is not that ſimple ſubſtance which the antiphlogiſtians ſuppoſe it to be; but that it contains phlogiſton. Had it been nothing more than a ſubſtance which had a ſtrong affinity to pure air, it would have united with the pure air from the turbith mineral, and have made vitriolic acid; but no vitriolic acid air would have been produced.

That vitriolic acid air contains the ſame inflammable principle with inflammable air is evident from the quantity of vitriolic acid air which I produced by reviving copper from blue vitriol in inflammable air. See my Experiments, vol. VI. p. 15. Mr. KIRWAN alſo produced this air from ſulphur and red precipitate. See his Treatiſe on Phlogiſton, p. 29.

When I uſed a ſmall quantity of ſulphur in proportion to the turbith mineral, the firſt produce was vitriolic acid air, and afterwards dephlogiſticated air, from the turbith mineral alone, the effect of the ſulphur having been exhauſted.

According to the antiphlogiſtic theory, *phoſphorus*, as well as ſulphur, is a ſimple ſubſtance; and when it is ignited imbibes pure air, and thereby becomes the phoſphoric acid, without parting with any thing. But I find, that after the accenſion of it in dephlogiſticated air, there is a conſiderable quantity of fixed air in the reſiduum; and this fixed air could only be formed by the union of the dephlogiſticated air in the veſſel with the phlogiſton contained in the phoſphorus. Mr. KIRWAN had a ſimilar reſult from phoſphorus confined in atmoſpheric air. As it is not pretended, that there is any plumbago in phoſphorus, this experiment is not liable to the objection that has been made to thoſe in which inflammable air from iron was made uſe of.

It will be expected, that in this reply to the objections that have been made to my experiments eſtabliſhing the doctrine of phlogiſton, I ſhould conſider what has been alledged by Meſſ. LAVOISIER, BERTHOLLET, and DE FOURCROY, in favour of their new ſyſtem, in their *Report* on the ſubject of the new chemical characters invented by Meſſ. HASSENFRATZ and ADET, ſubjoined to the new *Nomenclature Chymique.* I ſhall therefore notice what appears to me to be moſt important in that publication.

"One of the articles of the modern doctrine" (of which they ſay, p. 311. "that it coſt more than twenty years labour, which "the force of reaſoning has obliged many celebrated chemiſts to "adopt, and in favour of which much greater numbers are ready "to decide;" and the evidence for which they ſay, p. 301. "is the moſt complete chemical proof), which ſeems the "moſt ſolidly eſtabliſhed," p. 298, "is the formation, the "decompoſition, and recompoſition of water; and how is "it poſſible," they add, "to doubt of it, when we ſee that, "in burning together 15 grains of inflammable air and 85 of "pure air, we get exactly 100 grains of water; and when we "can, by decompoſition, find again theſe ſame two principles, "in the ſame proportions?"

To this I muſt ſay, as I have done, Experiments, vol. VI. p. 139. (and when I wrote that, I was myſelf a believer in the decompoſition of water), that I have never been able to find the full weight of the air decompoſed in the water produced by the decompoſition; and that now I apprehend it will not be denied, that the produce of this decompoſition is not mere water, but always ſome acid.

As to the ſuppoſed decompoſition of water by means of iron, I have ſhewn that it is a fallacy; ſince the iron imbibes nothing but water when it parts with its phlogiſton. And I have obſerved (Experiments, vol. VI. p. 83.), that when this finery cinder is reconverted into iron by inflammable air, nothing but water is expelled from it; and that the reſiduum of the air is purely inflammable, without containing any fixed air. It is evident, therefore, that the iron had imbibed pure water only. Had the iron imbibed dephlogiſticated air from the water, and not water itſelf, there ſeems to be no reaſon why fixed air ſhould not be found in this, as well as in the exactly ſimilar proceſs with minium and precipitate *per ſe.* Alſo, it can never be ſuppoſed, that the addition which iron gains, of one-third of its weight, is from air contained in ſteam, if it could be proved to contain any; becauſe, if there be a ſufficient quantity of iron, the whole of the water will be imbibed; ſo that, on this hypotheſis, water muſt be nothing but dephlogiſticated air condenſed.

There is, I acknowledge, a great difficulty in explaining the experiment of iron firſt imbibing water, and parting with phlogiſton, and again parting with its water, and imbibing phlogiſton, in circumſtances of heat ſo nearly ſimilar as thoſe which I have deſcribed. It ſeems as if the affinity of iron to water and to phlogiſton was each, in their turns, ſtronger than the other. To this I can only ſay, that the whole doctrine of affinities, as far as it is true, is founded on facts; and theſe are clearly ſuch as I have repreſented; and that a difference of circumſtances, which is not apparent at preſent, may become ſo when we ſhall have given ſufficient attention to them.

In order to ſatisfy myſelf whether any thing beſides *water* was expelled from finery cinder by heat, I went through ſimilar proceſſes with this ſubſtance and *maſſicot*, from which all air had been previouſly expelled; and after reviving both of them in inflammable air, I found the reſults, in all reſpects, the very ſame. The reſiduums of the inflammable air were equally free from fixed air; and when they were fired with equal quantities of dephlogiſticated air, the diminutions of bulk were very nearly the ſame, leſs than when the original inflammable air was uſed, becauſe all the impurities in the whole quantity were retained in a ſmall reſiduum, the metals having imbibed nothing but pure phlogiſton. Alſo the inflammable air had been long confined by water, in conſequence of which it is always altered more or leſs. The particulars of the proceſſes were as follows:

The finery cinder was revived in 7 oz. m. of inflammable air, which was thereby reduced to 1¾ oz. m.; and an oz. m. of this reſiduum being fired together with an equal quantity of dephlogiſticated air, not very pure, the diminution of both was to 28 diviſions of a tube, of which 30 was one oz. m. when with equal quantities of the ſame dephlogiſticated and the original inflammable air the diminution was to 18.

The maſſicot was reduced in 8 oz. m. of inflammable air till it was reduced to 1¼ oz. m.; and after the proceſs with the dephlogiſticated air, the diminution was to 29, when with the original inflammable air it was to 17½.

In both the reſiduums, after the exploſion, there was a ſlight appearance of *fixed air*, though none could be perceived before the exploſion; but in both caſes it was ſo ſlight that it could not have been perceived by the diminution of its bulk. But ſince both fixed air and nitrous acid are produced from the ſame materials in different circumſtances, it cannot be thought extraordinary if, in ſome caſes, both ſhould be produced at the ſame time.

M. Lavoisier and his aſſociates farther obſerve, p. 300. with reſpect to my experiments, that "when a calx is revived "in inflammable air, more water is found in the veſſel than the "weight of inflammable air that diſappears, ſo that it could "not have been contained in that air." They only refer to my experiments in general; but as they ſpeak of the water produced as appearing both on the inſide of the veſſel, and on the ſurface of the mercury, it can be no other than the experiment of the revival of iron from finery cinder; and the water that is found in this proceſs was never ſuppoſed to come from the little that is contained in the inflammable air, but the much greater quantity contained in the cinder.

Before I conclude this Paper, I ſhall juſt mention a few circumſtances attending the many exploſions I have made of inflammable and dephlogiſticated air in the long metallic and glaſs veſſels I have made uſe of, as they were pretty remarkable. The exploſions were made by a ſmall electric ſpark at one end of the veſſel, and the greateſt force of the exploſion was always at the other end. No tinned iron veſſel could bear many of them before they ſwelled out at that end, and at length burſt; and even the flat end of the copper veſſel, which was not leſs than one-tenth of an inch thick, was in time made quite convex, and the cylindrical part next to it was made very ſenſibly wider than any other part of the tube. This muſt have been effected by mere *force*, and not by *heat*; for the hotteſt part of the tube, after every exploſion, was never there, but always about the middle, though ſomething nearer to that end than the other, and in the glaſs veſſel the denſe cloud was always formed at that end.

The probability is, that the air where the electric ſpark is made taking fire firſt, the inflammation does not extend itſelf ſo rapidly but that the air at the oppoſite end is firſt condenſed, in conſequence of the inflammation and expanſion of the air at the other end, ſo that the air is there fired in a condenſed ſtate; and hence its greater force.

III. *Obſervations on the Claſs of Animals called, by* Linnæus, *Amphibia; particularly on the Means of diſtinguiſhing thoſe Serpents which are venomous, from thoſe which are not ſo.* *By* Edward Whitaker Gray, *M. D. F. R. S.*

Read December 18, 1788.

OF the various claſſes of the animal kingdom, no one has been ſo little attended to as the claſs, called by LINNÆUS, Amphibia. What he himſelf did in that claſs (though far ſuperior to what any other perſon has done) was evidently done in a hurry; falſe references are, at leaſt, as common in that, as in any other part of his works, and many of his deſcriptions are given in a very careleſs manner; others there are, however, which are truly worthy of their author, and in which the ſpecific characters are pointed out with that clearneſs and preciſion, which ſo eminently diſtinguiſh the deſcriptions of LINNÆUS from thoſe of all his predeceſſors.

In the conſtruction of the claſs, LINNÆUS has been particularly unfortunate; as he has erred, not only in making an unilocular heart one of the characters of it, but alſo in making the cartilaginous fiſhes a part of it. I think it needleſs to mention the cauſes which led him to this latter error; every anatomiſt now agrees that the Amphibia Nantes are not furniſhed with lungs; and every naturaliſt is convinced of the propriety of removing them, from the claſs of Amphibia, to that of Fiſhes. I ſhall only obſerve that, by the removal, the name of the claſs (which ſome naturaliſts have cavilled at) becomes much leſs objectionable; there being few genera, in the two orders of which it is now preſumed to conſiſt, which do not contain animals to which the term amphibious may, with ſome propriety, be given; whereas, in the order of Nantes, not one ſpecies occurs which has the ſmalleſt claim to that title. With reſpect to the other error I noticed (*viz.* that of ſuppoſing the hearts of the Amphibia to be ſingle) it would be eaſy to ſhew that it was not an uncommon one, at the time LINNÆUS formed his ſyſtem. And indeed he appears to have been led into it, by following an author whom he probably ſuppoſed of too great fame not to be ſafely relied on. At leaſt, in defence of his opinion, he quotes the following words of BOERHAAVE. "*In omnibus animalibus in quibus ſanguis non calet, ventriculus cordis eſt unicus.*" Whether the hearts of all the different genera, of which the claſs is compoſed, have yet been accurately examined; and whether an exact ſimilarity of ſtructure is found throughout the claſs; are queſtions I do not mean, at preſent, to examine. It is ſufficient for my purpoſe to obſerve, that the hearts of moſt of the Amphibia are now well known to be double, with an immediate communication between the two cavities; which ſtructure ſeems peculiarly adapted to that change of element, which (as I before obſerved) many of them can, for a time, ſupport; and thereby furniſhes another argument in favour of the name LINNÆUS has given to the claſs.

To conſider the ſtructure of the heart, however, is not abſolutely neceſſary in forming the characters of the claſs: the animals of which it conſiſts being ſufficiently diſtinguiſhed from all others, by having cold red blood, and breathing by means of lungs. Theſe two characters render the claſs perfectly diſtinct from the reſt; the two ſuperior ones, *viz.*

Mammalia and Birds, having warm blood; and the three inferior ones, *viz.* Fiſhes, Inſects, and Worms, not being furniſhed with lungs.

In his generic characters, LINNÆUS has been more ſucceſsful than in thoſe of the claſs; inſomuch that they may, I think, be conſidered as the beſt hitherto given. Whoever will be at the pains of comparing LINNÆUS's genera of Amphibia with thoſe of GRONOVIUS, will find, that the generic characters of the former, though few in number, are preciſe and diſtinct; while thoſe of the latter, though more numerous, are vague, indiſtinct, and ſometimes inaccurate. As a glaring inſtance of inaccuracy, I need only refer to the Chamæleon, which by GRONOVIUS is made a diſtinct genus, of which one of his characters is, *Pedes unguibus deſtituti*; whereas, in fact, the feet of that animal are furniſhed with very diſtinct, and pretty large, claws.

But though LINNÆUS's genera of Amphibia are, upon the whole, well formed, it muſt be allowed to be a great imperfection in them, that the venomous ſerpents are not ſeparated from the others.

From ſome expreſſions of his, in the Preface to the *Muſeum Regis*, and in the Introduction to the Claſs Amphibia, in the *Syſtema Naturæ*, it ſeems, that he thought it not eaſy to diſtinguiſh them, by any external characters; and his ideas reſpecting the venomous fangs themſelves were (as we ſhall ſee hereafter) ſo vague and confuſed, that it was hardly poſſible for him to attempt to found a generic diſtinction upon them *.

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* As a ſort of comparative excuſe for LINNÆUS, it may be obſerved, that GRONOVIUS (though he made two more genera of Serpents than LINNÆUS) did not ſeparate the venomous ones; neither has he diſtinguiſhed them by a mark (as LINNÆUS has) or by any other means.

Whether venomous Serpents can be, with certainty, diſtinguiſhed from others, and if ſo, how they are to be known, is what I mean to conſider in this Paper; in doing which I ſhall examine, firſt, how far they may be diſtinguiſhed by any external characters; ſecondly, ſuppoſing the venomous fangs to be the only certain criterion, how thoſe fangs are to be diſtinguiſhed from common teeth.

Though Serpents, by their internal organization, naturally belong to the third claſs of the animal kingdom, they are, in their external form, more ſimple than moſt of the animals belonging to the three inferior claſſes; their external characters muſt conſequently be very few. I ſhall firſt examine thoſe of the head; and, as all venomous Serpents (ſo far as our preſent experience extends) are contained in the three firſt of LINNÆUS's genera, I ſhall, at preſent, conſider only thoſe three.

In the firſt genus, Crotalus, the head is broader than the neck, depreſſed or flat at top, and covered with ſmall ſcales. Theſe three characters are particularly obſervable in the three intermediate ſpecies horridus, Dryinas, and Duriſſus. In the miliarius the ſcales of the head are rather larger than in the others. The mutus I have never ſeen; but it certainly ſhould not be placed among the Crotali *.

As all the ſpecies of this genus are venomous, one is naturally led, by the examination of it, to conſider the forementioned characters as being, in ſome meaſure, proper to venomous ſerpents. In order to ſee how far they are ſo, I ſhall, for the preſent, paſs over the next genus, Boa, and conſider that

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* LINNÆUS's reaſon for not placing it among the Boæ ſeems to have been, that he ſuppoſed none of them were venomous. He appears, however, to have had his doubts about the contortrix. I have examined it, and am convinced it is venomous.

that which follows it, Coluber. In that genus are many venomous ſpecies, and it is very certain that, in general, they have the fore-mentioned characters; examples of which may be ſeen in the Atropos, Ceraſtes *, atrox, Berus, and others. It is, however, equally certain, that there are ſome in which they are not to be found. As an example of this, I need only mention the Naja, a ſpecies well known to be very venomous; the head of which is neither depreſſed nor broad, is covered with large ſcales, and is, in every reſpect, a complete exception to what has been ſaid, reſpecting the heads of venomous Serpents.

Since then, there are venomous Serpents in which the fore-mentioned characters, *viz.* a broad and depreſſed head, covered with ſmall ſcales, are not to be found; I ſhall next examine whether thoſe characters are to be found in any of thoſe Serpents which are not venomous. In the genus Coluber there are very few (except venomous ones) which have the head much broader than the neck; and of thoſe few, I believe, every one has the head covered with large ſcales. But in the genus Boa, though no ſpecies is venomous, except the contortrix, almoſt every one has the head broad, depreſſed, and covered with ſmall ſcales. The canina, Conſtrictor, hortulana, beſides ſome others not deſcribed by LINNÆUS, furniſh examples of this. It muſt, however, be confeſſed, that the general character of the head of the Boa, though differing very widely from that of thoſe Colubri which are not venomous, is not quite that of the Crotalus; but the difference, though very obvious to a perſon accuſtomed to the examination of Serpents, is perhaps not eaſy to be fully expreſſed in words. It ſeems, however, to conſiſt principally in a lateral compreſſion, and elongation, of the anterior part of the head, ſo as to form a kind of ſnout. Hence the trivial name of *canina* is given by LINNÆUS to one of the ſpecies.

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* The Ceraſtes is not marked by LINNÆUS as a venomous ſpecies. He probably depended upon HASSELQUIST's deſcription, which I ſuſpect to have been made from a mutilated ſpecimen. Mr. ELLIS's deſcription in the Philoſophical Tranſactions, Vol. LVI. p. 287. is only a Tranſlation of HASSELQUIST's. But he obſerves, that Dr. TURNBULL told him it was venomous. That it is ſo, I have not the ſmalleſt doubt, though in the only ſpecimen I have ſeen of it the fangs were wanting. IMPERATO, who has given a figure of it (Hiſt. Nat. p. 784. Ed. Nap.), ſays it is very venomous.

From the characters of the head (as the trunk affords none deſerving conſideration) I ſhall proceed to thoſe of the other extremity.

In the Crotali I have never found the tail (excluſive of the Rattle) to exceed one-ninth part of the whole length; ſometimes I have found it much ſhorter. In ſome of the venomous Colubri, the proportion is ſtill leſs. In the Atropos I found it only one-thirteenth. In the Engliſh Viper (Coluber Berus) it is commonly about one-ſeventh or eighth. In ſome venomous ſpecies, however, the proportion is ſomething greater. In the Naja I have found it as much as one-ſixth; which proportion is, I believe, as great as I have ever obſerved: but that I may be ſure to keep within the truth, I will only ſay, that I have never met with a venomous Serpent, the tail of which was equal to one-fifth of the whole length*.

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* The tail of the Boa contortrix is ſaid by LINNÆUS to be one-third; but his own enumeration of the Scuta ſufficiently ſhews that this muſt be an error. The Coluber Leberis, Dipſas, and myćterizans, appear, by the number of ſcales under their tail, to furniſh exceptions to what I have ſaid. The two firſt I have never ſeen, but ſuſpect they are not venomous; that the laſt is not ſo I am very certain, having examined many ſpecimens of it.

With reſpect to thoſe Colubri which are not venomous, it muſt be confeſſed, that there are many whoſe tails are within the limits aſſigned to the venomous ones. In the Coluber Æſculapii, doliatus, getulus, and ſome others, the tail is not, in general, more than one-ſeventh of the whole length. In the lemniſcatus I have found it not exceeding one-twelfth or thirteenth; but I know no other Linnæan ſpecies in which it is ſo ſhort. In the greater number, however, the proportion of tail is more conſiderable; in many, it is full one-third. In the Ahætulla, and in ſome ſpecies not deſcribed by LINNÆUS, I have ſeen it more than two-fifths; but have never met with a ſpecies in which it was quite ſo long as the trunk, or half of the whole length.

I have not conſidered the Boæ, becauſe none of the Linnæan ſpecies, of that genus, have their tails either remarkably long, or ſhort; but, in two ſpecies, not deſcribed by LINNÆUS, I found the tail very little exceeding the proportion I have aſſigned to the Coluber lemniſcatus.

In the thickneſs of the tail, or in the acuteneſs of its termination, I have obſerved no difference worth remarking. In every ſpecies of the three firſt genera, the tail is thinner than the trunk; and in moſt of them it is more or leſs acute. The few exceptions I have obſerved were, I believe, none of them venomous; but they are too few to deſerve any particular conſideration.

A character of great uſe in diſtinguiſhing the ſpecies of Serpents, and which was not overlooked by LINNÆUS, is, that elevated line, or carina, with which the ſcales of many ſpecies are furniſhed. In order to ſhew how far this is to be conſidered as ſerving to diſtinguiſh venomous Serpents from others, I need only obſerve, that I have examined one hundred and twelve ſpecies of Serpents, not venomous, belonging to the three firſt genera; and find that eighty of them have ſmooth ſcales, and thirty-two only have carinated ones. Of venomous Serpents I have examined twenty-ſix; of which number, twenty have carinated ſcales, and only ſix have ſmooth ones. Upon the whole, therefore, carinated ſcales muſt be conſidered as being, in ſome meaſure, a character of venomous Serpents.

In what I have hitherto ſaid, I have conſidered only the three firſt genera of Serpents; I ſhall now make ſome remarks upon the three laſt.

Theſe three (*viz.* Anguis, Amphiſbæna, and Cæcilia), beſides the characters aſſigned them by LINNÆUS, have ſome others which are common to all, and which render them very different, in their external appearance, from any of the three firſt genera. Theſe are, a very thick and obtuſe tail, and a head which is very indiſtinct [*], and furniſhed with very ſmall eyes. This laſt character (*viz.* very ſmall eyes) is ſometimes, though very rarely, met with among the Colubri, for inſtance, in the lemniſcatus; in the three laſt genera, however, it takes place, I believe, without exception. The thickneſs of the tail is alſo common to every ſpecies; and though in the Anguis bipes, and in another ſpecies, not deſcribed by LINNÆUS, but figured in BROWNE's Hiſtory of Jamaica (Tab. XLIV. fig. 1. [†]), the tail has an acute termination, yet in both thoſe ſpecies, eſpecially in the laſt, it continues thick to the end, and becomes ſuddenly ſharp, being what in botanical language would be called, *obtuſa cum acumine.* With reſpect to the proportionate length of tail, however, it is very remarkable, that the genus

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[*] This indiſtinctneſs of the head, which is more or leſs common to each genus, is in the Amphiſbæna ſo conſiderable, as to have given riſe to the ſuppoſition of that Serpent's having a head at each end.

[†] This figure is, by LINNÆUS, erroneouſly quoted as his Anguis lumbricalis.

Anguis affords examples of much leſs proportion, and alſo of much greater, than is to be found in any of the three firſt genera. In the Anguis Scytale the tail is not above one-twentieth of the whole length; in the maculata it is not above one-fortieth; yet in the Anguis fragilis, and in the ventralis, the tail is always longer than the trunk, or, in other words, is more than half the whole length. Indeed, in one ſpecimen of the laſt mentioned ſpecies, I found the tail nearly two-thirds of the whole length. It may, however, be queſtioned whether that ſpecies is really an Anguis, or a Lacerta*.

I ſhall make no further remarks on the external characters of Serpents; the principal inferences to be deduced from thoſe I have already made, are the following.

1ſt, That a broad head, covered with ſmall ſcales, though it be not a certain criterion of venomous Serpents, is, with ſome few exceptions, a general character of them.

2dly, That a tail under one-fifth of the whole length, is alſo a general character of venomous Serpents; but, ſince many of thoſe which are not venomous have tails as ſhort, little dependance can be placed upon that circumſtance alone. On the other hand, a tail exceeding that proportion, is a pretty certain mark that the ſpecies, to which it belongs, is not venomous.

3dly, That a thin and acute tail is by no means to be conſidered as peculiar to venomous Serpents; though a thick and obtuſe one is only to be found among thoſe which are not venomous.

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* The Anguis ventralis of LINNÆUS, is ſo very like the Lacerta apoda, deſcribed by PALLAS, in Vol. XIX. of the Novi Comment. Petrop. as to render it doubtful whether it may not be the ſame. When I firſt examined it, I conſidered it as a Lacerta, on account of the projecting ſuture along the body, and the open ears; but I have ſince met with a ſpecimen which had two large echinated *Penes* (as they are called) a character which is, I believe, peculiar to Serpents.

4thly, That carinated ſcales are, in ſome meaſure, characteriſtic of venomous Serpents, ſince in them they are more common than ſmooth ones, in the proportion of nearly 4 to 1; whereas, ſmooth ſcales are, in thoſe Serpents which are not venomous, more common, in the proportion of nearly 3 to 1.

Upon the whole therefore it appears, that though a pretty certain conjecture may, in many inſtances, be made, from the external characters; yet, in order to determine, with certainty, whether a Serpent be venomous or not, it becomes neceſſary to have recourſe to ſome more certain diagnoſtic. This can only be ſought for in the mouth; I ſhall therefore next conſider, how the fangs, with which the mouths of venomous Serpents are furniſhed, are to be diſtinguiſhed from common teeth.

To thoſe who form their ideas of the fangs of venomous Serpents, from thoſe of the Rattle-ſnake, or even from thoſe of the Engliſh Viper, it will appear ſtrange, that there ſhould be any difficulty in diſtinguiſhing thoſe weapons from common teeth; and indeed the diſtinction would really be very eaſy, were all venomous Serpents furniſhed with fangs as large as thoſe of the fore-mentioned ſpecies. But the fact is, that in many ſpecies the fangs are full as ſmall as common teeth, and conſequently cannot, by their ſize, be known from them; this is the caſe with the Coluber laticaudatus *, lacteus, and ſeveral others, I cannot, however, better demonſtrate that the diſtinction, between the venomous fangs and common teeth, is not very obvious, than by ſhewing how very vague and erroneous

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* This ſpecies is by LINNÆUS reckoned venomous, in the Muſeum Regis, though the mark is not affixed to it in the Syſtema Naturæ. To me it appears to be certainly venomous, and is the only water Serpent I have met with that is ſo.

LINNÆUS's ideas about them were; nor can I better prove the want of information on this ſubject, than by obſerving that, erroneous as the ideas of LINNÆUS were, no one, that I know of, has yet attempted to furniſh more correct ones.

LINNÆUS thought the fangs might be diſtinguiſhed by their mobility; this, at leaſt, may be fairly inferred, from his never mentioning them in the Muſeum Regis, without adding the epithet *mobilia*, except in one inſtance (the Coluber aulicus); and, in that very inſtance, the want of mobility in the ſuppoſed fangs appears evidently to raiſe doubts in his mind, whether they are really fangs or not. His words are, "*Dentes, ſive tela, duo, rigida, parva, non mobilia.*" Theſe doubts, reſpecting the above-mentioned ſpecies, I am not able to remove, as I am not ſure that I have ever ſeen it [*]. But with regard to mobility, conſidered in general as a character of venomous fangs, I muſt aſſert, not only that I have never found it ſo, but alſo, that I have never been able to diſcover in them any thing which I thought could properly be called mobility: I have, indeed, ſometimes found ſome of them looſe in their ſockets; but then I have found others, in the ſame ſpecimen, quite fixed. The ſame thing was obſerved both by Dr. NICHOLLS [†], and by the Abbé FONTANA [‡], in the common Viper, even during life. The looſe fangs may be ſuch as have not yet been firmly fixed in their ſocket, or they may have been looſened by ſome accident: for I ſuſpect that the fangs may be at any time looſened, and even diſplaced, by a ſmall degree of violence; and that, perhaps, may be one reaſon why there is always a certain number of ſmall fangs, near the baſe of the full grown ones, ready to enlarge and take their place, if they ſhould be, by any accident, torn out.

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[*] I have ſeen one, which agreed pretty well with LINNÆUS's deſcription; if that was really his ſpecies, it is not venomous.

[†] Appendix to Dr. MEAD's Account of the Viper.

[‡] FONTANA, Traité ſur le Venin de la Vipere, chap. 1ſt and 2d.

LINNÆUS ſeems alſo to have thought that the fangs might be known by their ſituation. In the Introduction to the claſs Amphibia in the Syſtema Naturæ, he ſays they are, "*Dentibus* "*ſimillima ſed extra maxillam ſuperiorem collocata*;" and in the deſcription of the Crotalus Dryinas, in the Amœnitates Academicæ, he ſays, "*Dentes ejus duo canini uti in reliquis vene-* "*natis Serpentibus non in maxillis hærent, iis enim vulnerando,* "*non autem ictus infligendo utitur.*"

Theſe two quotations ſhew, that LINNÆUS thought the ſituation of the fangs different from that of the common teeth; the laſt alſo ſhews that he thought their mode of action influenced by it. What difference in ſituation may be found by accurate diſſection, it is foreign from my preſent purpoſe to enquire; I am, however, very certain that common examination * will not diſcover any difference, in that reſpect, between the fangs of venomous Serpents, and the teeth of others.

But the moſt ſingular opinion of LINNÆUS, reſpecting the venomous fangs, was, that they were ſometimes fixed in the baſe of the jaw. Of this he has given two inſtances in the Muſeum Regis. One in the deſcription of the Coluber ſeverus, of which he ſays, "*Haſtæ mobiles ſolitariæ verſus baſin maxil-* "*larum interius adhærent.*" The other in that of the Coluber ſtolatus. His words there are, "*Tela mobilia ad baſin maxil-*

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* By common examination I mean ſuch as may be made without diſſecting, or otherwiſe damaging, the ſpecimen to be examined; and ſuch only do I ſuppoſe allowable in the diſtinction I am ſeeking to eſtabliſh.

"*larum affixa, ut vix vulnerare valeat hoſtes, ſolum cibos veneno* "*inficere.*"

LINNÆUS's opinion reſpecting the uſe of the fangs, in the laſt mentioned ſpecies, appears to me not very clearly expreſſed *. But I have quoted both deſcriptions, merely to ſhew that LINNÆUS thought the fangs were ſometimes placed in the baſe of the jaw; an idea for which I have never been able to diſcover any foundation. The firſt of the two ſpecies in queſtion I have never ſeen; of the ſtolatus I have examined ſeveral ſpecimens, and am convinced it is not venomous.

I ſhall not dwell any longer on the falſe notions which have been entertained, reſpecting the fangs of venomous Serpents, but ſhall proceed to ſhew how, in my opinion, they may be moſt eaſily, and moſt certainly, diſtinguiſhed from common teeth.

With reſpect to their ſize, I have already obſerved that it is very various, conſequently no certain judgement can, in all caſes, be made from that circumſtance. In ſome ſpecies they are ſo large, that their ſize alone ſufficiently diſtinguiſhes them from common teeth; but in others they are ſo ſmall, that it is very difficult to diſcover them.

The ſize of the common teeth alſo varies very much, in different ſpecies. In the Coluber mycterizans they are remarkably large, eſpecially thoſe which are ſituated near the apex of the upper jaw; which circumſtance probably helped to lead LINNÆUS into the erroneous opinion he entertained, that this Serpent was venomous. But in many ſpecies the teeth are ſo ſmall, that it is impoſſible to diſcover, merely by looking into the mouth, that the animal has any. Yet in that caſe they may be very eaſily detected, by drawing a pin (or any other hard ſubſtance) with a moderate degree of preſſure, along the edge of the jaw, from the apex to the angle of the mouth, when they will be felt to grate againſt the pin, like the teeth of a ſaw.

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* LINNÆUS's opinion ſeems not unlike that of the Abbé FONTANA, who (in the work already mentioned, chap. 12.) ſuppoſes the poiſon of the Viper may be of uſe, to the animal, in digeſtion. To me the venomous fangs have ever appeared to be merely offenſive weapons; nor can I ſee greater difficulty in ſuppoſing ſuch a weapon, with the power of injecting poiſon, placed in the head of a Viper or Rattle-ſnake, than in ſuppoſing ſuch an one, with a ſimilar power, placed in the tail of a Waſp or Hornet.

Although the ſize of the venomous fangs is very various, their ſituation is, I believe, always the ſame; namely, in the anterior and exterior part of the upper jaw, which ſituation I conſider as the only one in which venomous fangs are ever found. But as, in thoſe Serpents which are not venomous, common teeth are found in that part of the jaw, it is plain that we cannot, by ſituation alone, diſtinguiſh one from the other. They may, however, be diſtinguiſhed with great eaſe, and I believe alſo with great certainty, by the following ſimple operation. When it is diſcovered that there is ſomething like teeth in the fore-mentioned part of the upper jaw, let a pin be drawn, in the manner already deſcribed, from that part of the jaw to the angle of the mouth (which operation may, for greater certainty, be tried on each ſide). If no more teeth are felt in that line, it may I believe be certainly concluded, that thoſe firſt diſcovered are what I have diſtinguiſhed by the name of fangs, and conſequently, that the Serpent is a venomous one *. If, on the contrary, the teeth firſt diſcovered are found not to ſtand alone, but to be only a part of a complete row, it may as certainly be concluded, that the Serpent is not venomous.

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* If a ſpecimen ſhould be met with, in which no teeth, of any kind, can be diſcovered in the margin of the upper jaw, the preſumption is, that it is a venomous Serpent, which has loſt its fangs; but I have never met with ſuch an one, except the Coluber Ceraſtes already mentioned.

In the upper jaw, both of venomous Serpents and others, beſides the teeth already ſpoken of, there are two interior rows; conſequently, the diſtinction I have endeavoured to eſtabliſh might be expreſſed in other words, by ſaying, that all venomous Serpents have only two rows of teeth, in the upper jaw, and all others have four[*]. I think it better, however, to leave the interior rows out of the queſtion, as, in many ſpecies, the teeth of which they are compoſed are ſo ſmall, as to make it very difficult to diſcover them. Indeed, in two ſpecies of Anguis, I can hardly be ſure that I have diſcovered them; but as, in every other ſpecies, I have never failed to do ſo, I preſume I may, with very little riſk of error, aſſert, that all Serpents whatever are furniſhed with them; and that thoſe only, which are not venomous, have the exterior rows.

What I have ſaid ſufficiently ſhews that LINNÆUS's ideas, reſpecting venomous ſerpents, were ſuch as did not permit him to ſeparate them from the others; if the method I have propoſed ſhall be found to render the diſtinction of them ſufficiently clear and eaſy, it naturally follows, that they ſhould be made generically diſtinct. Some other reforms might alſo be made in LINNÆUS's claſs of Amphibia, the conſideration of which I do not mean, at preſent, to enter further into. But, before I conclude, I think it neceſſary to notice an inaccuracy of LINNÆUS, of a different kind from thoſe I have already pointed out.

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[*] GRONOVIUS, of whoſe inaccuracy I have already given one inſtance, in deſcribing the Crotalus Duriſſus, in his Muſeum Ichthyologicum, ſays it has no teeth, except the venomous fangs. KLEIN, in his Tentamen Herpetologiæ, has gone ſtill further, having actually made a genus of Serpents without teeth, which he calls Anodon. He appears not to have examined the mouth of a ſingle ſpecies; but to have depended intirely upon the deſcriptions of SEBA.

In the Preface to the Muſeum Regis, and in the Introduction to the claſs Amphibia, in the Syſtema Naturæ, LINNÆUS ſays, that the proportion of venomous Serpents to others, is 1 in 10; yet in the Syſtema Naturæ, in which the ſum total of ſpecies is one hundred and thirty-one, he has marked twenty-three as venomous, which is ſomewhat more than 1 in 6. How he came to be ſo much at variance with himſelf, I know not; but the laſt mentioned proportion ſeems to me to be not far from the truth; as I find that I have examined one hundred and fifty-four ſpecies of Serpents, of which number twenty-ſix appear to be venemous.

I have already mentioned, that the Coluber ſtolatus and the myčterizans, though marked by LINNÆUS as venomous Serpents, certainly are not ſo; and that I ſuſpect the ſame may be ſaid of the Leberis, and Dipſas. I have alſo obſerved, that the Boa contortrix, Coluber Ceraſtes, and laticaudatus, none of which are marked in the Syſtema Naturæ, are all of them venomous; to theſe laſt may be added the Coluber fulvus.

If LINNÆUS's ſpecies were all accurately examined, I have no doubt but more errors, of both kinds, would be found; for it muſt be obſerved, that though I have examined a greater number of ſpecies than LINNÆUS, not above half that number are of thoſe deſcribed by him; conſequently there remains more than one-third of his ſpecies which I have never ſeen. The number I have examined, however, ſeems to me ſufficiently great to warrant the inferences I have drawn from that examination. That ſome exceptions to them might be found, by the examination of a greater number, is very poſſible; but, if theſe obſervations ſhall tend to rectify the falſe notions which have been entertained reſpecting venomous Serpents, and to render the diſtinction between them and others more clear, I truſt they will be thought not totally uſeleſs.

IV. *Obſervations on the Dryneſs of the Year* 1788. *In a Letter from the Rev. Mr.* B. Hutchinſon *to Sir* Joſeph Banks, *Bart. P. R. S.*

Read January 15, 1789.

S I R, Kimbolton, January 8, 1789.

AS the defect of rain has been very conſiderable in 1788; and in conſequence a great want of water on the cloſe of the year univerſally felt; perhaps the quantity fallen here, compared with that of the ſeven preceding years, may not be unacceptable to yourſelf and the Royal Society.

		Inches.	
Rain	1781	21,6	
	1782	32,3	
	1783	23,6	
	1784	28,0	$\frac{175}{5} = 25$ inches, the mean of ſeven years.
	1785	21,0	
	1786	24,7	
	1787	23,8	
	1788	14,5	

By eſtimation it therefore appears, that the average quantity of rain of the ſeven preceding years is 25 inches, and the rain which fell laſt year is only 14,5, that is, not much more than half that quantity, if we deduct 1,3 now lying in ſnow, which fell in December, and not in ſolution. On the ſuppoſition which, I believe, is not far from truth, that the whole iſland has had the ſame defect; a greater failure of the produce of the earth might have been expected than what the country has experienced; for, except in hay, and a little failure in turneps, the crops have in general been as plentiful as in moſt of the former years, and in fruits of the orchard much more ſo.

It has always been ſaid of England, that drought never occaſions want; this year verifies the aſſertion. But to account for crops that, taken on the whole, are rather abundant, we may conſult the following monthly ſtate of rain for 1788.

	Inches.
In January	0,3
February	1,7
March	0,7
April	0,0
May	0,6
June	1,8
July	0,8
Auguſt	3,4
September	3,4
October	0,3
November	0,2
December	1,3
	14,5

Having premiſed, Sir, that there were no extremes of cold and heat throughout the year; the thermometer in a northern expoſure never falling below the freezing point during the day-time, except on the 14th and 15th of January, the 6th, 7

7th, 8th, 10th, 11th, 12th, 13th, and 17th of March, and on none of thoſe days at noon, ſo that there never were twenty-four hours together ſucceſſive froſt; therefore vegetation was never entirely at a ſtand. In ſummer it did not riſe to 80 degrees, except on

		Deg.	
May	26	80	
	27	81	
	28	81	
June	18	83	with thunder and rain: then cool for a week.
	27	80	
July	11	80	
	12	82	
Auguſt	4	81	: the reſt of the time exceedingly temperate.

Now, the rain that fell on February was towards the end of the month; which, together with that which fell in March, brought up the ſpring corn, gave an early firſt crop of hay to the large towns, and covered the meadows and paſtures in the country; that they were not ſo entirely dried up through the defect of April, as to prevent the rain, which fell plentifully on the 29th of May, ſucceeded by more in June, giving a ſecond crop to the former ſituations, and a firſt, though late one, to the latter: and as fructification chiefly depends on rain falling at the latter end of the ſeaſon of flowering, this rain ſet the bloſſoms of wheat, and of the uſeful fruit-trees; as the great rains in Auguſt ſwelled the kernel, filled, as they term it, the buſhel, and gave an opportunity for a ſecond crop of turneps that proved more vigorous than the firſt.

* * *

V. *On the Method of determining, from the real Probabilities of Life, the Value of a contingent Reverſion in which Three Lives are involved in the Survivorſhip. By Mr.* William Morgan; *communicated by the Rev.* Richard Price, *D. D. F. R. S.*

Read January 29, 1789.

IN a Paper which I had lately the honour of communicating to the Royal Society, reſpecting the method of determining the values of reverſions depending on ſurvivorſhips between two perſons from the real probabilities of life, I obſerved, that the inveſtigation of thoſe caſes in which three lives were involved in the ſurvivorſhip (though attended with much more difficulty) might, however, be effected in a ſimilar manner. The further purſuit of this ſubject has now convinced me that, as it is never ſafe, ſo likewiſe it can never be neceſſary to have recourſe to the *expectations* of life in any caſe; and that the ſolution even of thoſe problems which include three lives is far from being ſo formidable as at firſt ſight it appears to be. I am ſenſible of the impropriety of entering minutely in this place into the vaſt variety of propoſitions which refer to the different orders of ſurvivorſhip between three lives; but as the following problem ſeems to be of conſiderable importance on account of its being applied to the ſolution of many other problems, the demonſtration of it, perhaps, may not be thought an improper addition to my former Paper.

PROBLEM.

Suppoſing the ages of A, B, and C, to be given; to determine, from any table of obſervations, the value of the ſum S payable on the contingency of C's ſurviving B, provided the life of A ſhall be then extinct.

SOLUTION.

Let a repreſent the number of perſons living in the table at the age of A. Let a', a'', a''', a'''', &c. repreſent the decrements of life at the end of the 1ſt, 2d, 3d, 4th, &c. years from the age of A. Let b repreſent the number of perſons living at the age of B, and m, n, v, p, &c. the number of perſons living at the end of the 1ſt, 2d, 3d, 4th, &c. years from the age of B. In like manner let c repreſent the number of perſons living at the age of C, and d, e, f, g, &c. the number of perſons living at the end of the 1ſt, 2d, 3d, 4th, &c. years from the age of C. Let r alſo denote the value of £. 1 increaſed by its intereſt for a year. In order to receive the ſum S in the firſt year, it is neceſſary either that all the three lives ſhall have died in that year, A having died firſt, B next, and C laſt; or that only the two lives A and B ſhall have died (A having died firſt), and that C ſhall have lived to the end of that year. The probability that the three lives ſhall die in the firſt year is $\frac{a' . \overline{b-m} . \overline{c-d}}{abc}$. The probability that they ſhall die in the order above mentioned is $\frac{a' . \overline{b-m} . \overline{c-d}}{6 . abc}$. The probability that both A and B ſhall die in the firſt year is $\frac{a' . \overline{b-m}}{ab}$. Half this fraction, or $\frac{a'\cdot\overline{b-m}}{2ab}$, is the probability that the death of A ſhall happen before the death of B in this year. The probability that C ſhall ſurvive A and B, reſtrained to the contingency of A's having died firſt, is $\frac{a'\cdot\overline{b-m}\cdot d}{2abc}$. The value therefore of the ſum S for the firſt year is $S\times\overline{\frac{a'\cdot\overline{b-m}\cdot\overline{c-d}}{6\cdot abcr}+\frac{a'\cdot\overline{b-m}\cdot d}{2abcr}}=\frac{S}{abcr}\times\overline{\frac{a'bc}{6}-\frac{a'mc}{6}+\frac{a'db}{3}-\frac{a'md}{3}}$ . . . . In the ſecond year the payment of the given ſum will depend on either of four events happening. Firſt, on the contingency of all the three lives dying in that year, A having died firſt, B next, and C laſt. 2dly, On the contingency of B's dying in that year, C's living to the end of it, and A's dying in the firſt year. 3dly, On the contingency of B's dying after A in the ſecond year (both of them having ſurvived the firſt year) and of C's living to the end of that year. 4thly, On the contingency of A's dying in the firſt year, and of B and C's both dying in the ſecond year, B having died firſt. The probability of the firſt contingency is expreſſed by the fraction $\frac{a''\cdot\overline{m-n}\cdot\overline{d-e}}{6\cdot abc}$. The probability of the ſecond by the fraction $\frac{a'\cdot\overline{m-n}\cdot e}{abc}$. The probability of the third by the fraction $\frac{a''\cdot\overline{m-n}\cdot e}{2\cdot abc}$. And the probability of the fourth contingency by the fraction $\frac{a'\cdot\overline{m-n}\cdot\overline{d-e}}{2abc}$. Theſe ſeveral fractions, therefore, multiplied into $\frac{S}{r^2}$ will be the value of the given ſum for the ſecond year, and may be eaſily found $= \frac{S}{abcr^2}\times\overline{\frac{a''dm}{6}-\frac{a''dn}{6}+\frac{a''em}{3}-\frac{a''en}{3}+\frac{a'em}{2}-\frac{a'en}{2}+\frac{a'dm}{2}-\frac{a'dn}{2}}$. In like manner the payment of the given ſum in the third year will depend on the contingency of the ſame number of events as in the ſecond year; that is, it will, firſt, depend on the contingency of all the three lives dying in that year, A having died firſt, B next, and C laſt; 2dly, on the contingency of B's dying in that particular year, C's living to the end of it, and A's dying in the firſt or ſecond years; 3dly, on the contingency of B's dying after A in the third year (both of them having ſurvived the two preceding years), and of C's living to the end of that year; and, 4thly, on the contingency of A's dying in the firſt or ſecond year, and of B and C's both dying in the third year, C having died laſt. Theſe ſeveral contingencies are expreſſed by the reſpective fractions $\frac{a''.\overline{n-o}.\overline{e-f}}{6.abc}$ . . . $\frac{\overline{a'+a''}.\overline{n-o}.f}{abc}$ . . . $\frac{a''.\overline{n-o}.f}{2abc}$ . . . and $\frac{\overline{a'+a''}.\overline{n-o}.\overline{e-f}}{2abc}$. Conſequently the value of the ſum S for the third year will be $= \frac{S}{abcr^3} \times \overline{\frac{a'''.en}{6} - \frac{a'''.eo}{6} + \frac{a'''.fn}{3} - \frac{a'''.fo}{3} +}$ $\overline{\frac{\overline{a'+a''}.fn}{2} - \frac{\overline{a'+a''}.fo}{2} + \frac{\overline{a'+a''}.en}{2} - \frac{\overline{a'+a''}.eo}{2}}$. And by reaſoning in the ſame manner the value of the ſum S for the fourth year may be found $= \frac{S}{abcr^4} + \overline{\frac{a''''.fo}{6} - \frac{a''''.fp}{6} + \frac{a''''.go}{3} - \frac{a''''.gp}{3} +}$ $\overline{\frac{\overline{a'+a''+a'''}.go}{2} - \frac{\overline{a'+a''+a'''}.gp}{2} + \frac{\overline{a'+a''+a'''}.of}{2} - \frac{\overline{a'+a''+a'''}.fp}{2}}$.

If either B or C be the oldeſt of the three lives, theſe ſeries continued to the extremity of that life will expreſs the whole value of the reverſion, which will be $= \frac{S}{6} \times$

$$\overline{\frac{a'bc}{abcr} + \frac{a''dm}{abcr^2} + \frac{a'''en}{abcr^3} + \frac{a''''.fo}{abcr^4} + \&c.} + \frac{S}{2r} \times \overline{\frac{a'dm}{abcr} + \frac{\overline{a'+a''}.en}{abcr^2} +}$$

$a'+a''$

$$\frac{\overline{a'+a''+a'''}\,.\,fo}{abcr^3}+\&c.-\frac{S}{6}\times\overline{\frac{a'mc}{abcr}+\frac{a''na}{abcr^2}+\frac{a'''oe}{abcr^3}+\frac{a''''\,.\,pf}{abcr^4}+\&c.}-$$

$$\frac{S}{2r}\times\overline{\frac{a'dn}{abcr}+\frac{\overline{a'+a''}\,.\,ea}{abcr^2}+\frac{\overline{a'+a''+a'''}\,.\,fp}{abcr^3}+\&c.}+\frac{S}{3}\times\overline{\frac{a'db}{abcr}+\frac{a''\,.\,em}{abcr^2}+}$$

$$\overline{\frac{a'''\,.\,fn}{abcr^3}+\frac{a''''\,.\,go}{abcr^4}+\&c}+\frac{S}{2r}\times\overline{\frac{a'em}{abcr}+\frac{\overline{a'+a''}\,.\,fn}{abcr^2}+\frac{\overline{a'+a''+a'''}\,.\,go}{abcr^3}+\&c.}$$

$$-\frac{S}{3}\times\overline{\frac{a'md}{abcr}+\frac{a''en}{abcr^2}+\frac{a'''\,.\,fo}{abcr^3}+\frac{a''''\,.\,gp}{abcr^4}+\&c.}-\frac{S}{2r}\times\overline{\frac{a'en}{abcr}+\frac{\overline{a'+a''}\,.\,fo}{abcr^2}+}$$

$$\overline{\frac{\overline{a'+a''+a'''}\,.\,gp}{abcr^3}+\&c.}$$

In order to ſum up the firſt and ſecond of theſe ſeries let β repreſent the number of perſons living at the age of F, a perſon one year younger than B, and κ the number of perſons living at the age of K, a perſon one year younger than C. Let FK, BC, AFK, and ABC, repreſent the value of an annuity on the two and three joint lives of F and K, of B and C, of A, F and K, and of A, B and C reſpectively; then will the ſeries $\frac{S}{6}\times\overline{\frac{a'bc}{abcr}+\frac{a''dm}{abcr^2}+\frac{a'''en}{abcr^3}+\&c.}$ be $=\frac{S\,.\,\beta\,.\,\kappa}{6\,.\,bc}\times$

$$\overline{\frac{bc}{\beta\kappa r}-\frac{\overline{a-a'}\,.\,bc}{a\beta\kappa r}+\frac{dm}{\beta\kappa r^2}-\frac{\overline{a-a'-a''}\,.\,dm}{a\beta\kappa r^2}-\frac{a'dm}{a\beta\kappa r^2}+\frac{en}{\beta\kappa r^3}-\frac{\overline{a-a'-a''-a'''}\,.\,en}{a\beta\kappa r^3}}$$

$$\overline{-\frac{\overline{a'+a''}\,.\,en}{a\beta\kappa r^3},\ \&c.}=\frac{S\,.\,\beta\kappa}{bc}\times\frac{\overline{FK-AFK}}{6}\left(-\frac{S}{6r}\times\overline{\frac{a'dm}{abcr}+\frac{\overline{a'+a''}\,.\,en}{abcr^2},\ \&c.}\right.$$

$$\left.=-\frac{S}{6r}\times\overline{\frac{dm}{bcr}-\frac{\overline{a-a'}\,.\,dm}{abcr}+\frac{en}{bcr^2}-\frac{\overline{a-a'-a''}\,.\,en}{abcr^2},\ \&c.}=\right)-\frac{S}{6r}\times$$

$\overline{BC-ABC}$. The ſum, therefore, of the two firſt ſeries, or of $\frac{S}{6}\times\overline{\frac{a'bc}{abcr}+\frac{a''\,.\,dm}{abcr^2}+\&c.}+\frac{S}{2r}\times\overline{\frac{a'dm}{abcr}+\frac{\overline{a'+a''}\,.\,en}{abcr^2}+\&c.}$ is $=\frac{S}{6}\times\frac{\beta\kappa\,.\,\overline{FK-AFK}}{bc}+\frac{S}{3r}\times\overline{BC-ABC}$. Again, let P repreſent a life one year older than B, and let BK, PC, ABK, and APC, repreſent the values of annuities on the two and three joint lives of B and

B and K, P and C, A, B and K and of A, P and C: then the ſum of the third and fourth ſeries, or of $-\frac{S}{6}\times\overline{\frac{a'mc}{abcr}+\frac{a''nd}{abcr^2}+\&c.}-\frac{S}{2r}\times\overline{\frac{a'dn}{abcr}+\frac{\overline{a'+a''}.eo}{abcr^2}+\&c.}$ being $=-\frac{S.\varkappa}{6c}\times\overline{\frac{mc}{\varkappa br}-\frac{\overline{a-a'}.mc}{ab\varkappa r}+\frac{dn}{b\varkappa r^2}-\frac{\overline{a-a'-a''}.dn}{ab\varkappa r^2}+\frac{eo}{b\varkappa r^3}-\frac{\overline{a-a'-a''-a'''}.eo}{ab\varkappa r^3},\&c.}$ $\left(-\frac{S}{3r}\times\overline{\frac{a'dn}{abcr}+\frac{\overline{a'+a''}.eo}{abcr^2},\&c.}=\right)-\frac{S.m}{3br}\times\overline{\frac{a'dn}{acmr}+\frac{\overline{a'+a''}.eo}{acmr^2}+\&c.}$, will be $=-\frac{S}{6}\times\frac{\varkappa.\overline{BK-ABK}}{c}-\frac{S}{3r}\times\frac{m.\overline{PC-APC}}{b}$. The fifth ſeries, $\frac{S}{3}\times\overline{\frac{a'db}{abcr}+\frac{a''em}{abcr^2}+\&c.}$, is $=\frac{S}{3}\times\frac{\beta}{b}\times\overline{\frac{db}{\beta cr}-\frac{\overline{a-a'}.db}{a\beta cr}+\frac{em}{\beta cr^2}-\frac{\overline{a-a'-a''}.em}{a\beta cr^2}+\frac{fn}{\beta cr^3}-\frac{\overline{a-a'-a''-a'''}.fn}{a\beta cr^3},\&c.}-\frac{S}{3r}\times\overline{\frac{a'em}{abcr}+\frac{\overline{a'+a''}.fn}{abcr^2},\&c.}$ = (putting FC and AFC for the values of the two and three joint lives of F and C and of A, F, and C) $\frac{S}{3}\times\frac{\beta.\overline{FC-AFC}}{b}-\frac{S}{3r}\times\overline{\frac{a'em}{abcr}+\frac{\overline{a'+a'}.fn}{abcr^2}}$, &c. The ſum, therefore, of the fifth and ſixth ſeries, or of $\frac{S}{3}\times\overline{\frac{a'db}{abcr}+\frac{a''em}{abcr^2},\&c.}+\frac{S}{2r}\times\overline{\frac{a'em}{abcr}+\frac{\overline{a'+a''}.fn}{abcr^2}+\&c.}$ is $=\frac{S}{3}\times\frac{\beta.\overline{FC-AFC}}{b}+\left(\frac{S}{6r}\times\overline{\frac{a'em}{abcr}+\frac{\overline{a'+a''}.fn}{abrc^2},\&c.}=\right)\frac{S\times d}{6cr}\times\overline{\frac{em}{bdr}-\frac{\overline{a-a'}.em}{abdr}+\frac{fn}{bdr^2}-\frac{\overline{a-a'-a''}.fn}{abdr^2}+\&c.}$ If T denote a life one year older than C, and BT, and ABT denote the values of the two and three joint lives of B and T and of A, B, and T, this laſt ſeries will be $=\frac{S}{6r}\times\frac{d.\overline{BT-ABT}}{c}$, and conſequently the ſum of the fifth and ſixth ſeries will be $=\frac{S}{3}\times\frac{\beta.\overline{FC-AFC}}{b}+\frac{S}{6r}\times\frac{d.\overline{BT-ABT}}{c}$. Laſtly, the ſeventh and eighth ſeries, or $-$

$\frac{S}{3}\times\overline{\frac{a'md}{abcr}+\frac{a''en}{abcr^2}+\&c.}-\frac{S}{2r}\times\overline{\frac{a'en}{abcr}+\frac{\overline{a'+a''}.fo}{abcr^2}+\&c.}$ are $=-\frac{S}{3}\times\overline{\frac{dm}{bcr}-\frac{\overline{a-a'}.dm}{abcr}+\frac{en}{bcr^2}-\frac{\overline{a-a'-a''}.en}{abcr^2}+\&c.}-\frac{S}{6r}\times\overline{\frac{a'en}{abcr}+\frac{\overline{a'+a''}.fo}{abcr^2}+\&c.}$ $=-\frac{S}{3}\times\overline{BC-ABC}-\left(\frac{S}{6r}\times\frac{md}{bc}\times\overline{\frac{en}{mdr}-\frac{\overline{a-a'}.en}{amdr}+\frac{of}{mdr^2}-\frac{\overline{a-a'-a''}.of}{amdr^2}+\&c.}=\right)\frac{S}{6r}\times\frac{md.\overline{PT-APT}}{bc}$, where PT and APT repreſent the values of the two and three joint lives of P and T, and of A, P, and T. If theſe ſeveral expreſſions be added together, &c. we ſhall at laſt have $\frac{S.x}{6c}\times\overline{\frac{\beta.\overline{FK-AFK}}{b}-\overline{BK-ABK}}$ $+\frac{S.\beta}{3b}\times\overline{FC-AFC}-\frac{S.\overline{r-1}}{3r}\times\overline{BC-ABC}-\frac{S.m}{3br}\times\overline{PC-APC}+\frac{S.d}{6cr}\times\overline{\overline{BT-ABT}-\frac{m.\overline{PT-APT}}{b}}$, for the value of the ſum S, when either B or C are the oldeſt of the three lives.

In order to determine the value of the reverſion when the life of A is the oldeſt of the three lives, let s, t, u, w, &c. be the number of perſons living at the end of the 1ſt, 2d, 3d, 4th, &c. years from the age of A, and let b', b'', b''', b'''', &c. be the decrements of life at the end of 1, 2, 3, 4, &c. years from the age of B; then, by reaſoning as above, the value of the ſum S for the firſt year will be expreſſed by the ſeries $\frac{S\times b'.\overline{a-s}.\overline{c-d}}{6abcr}+\frac{S\times b'.\overline{a-s}.d}{2abcr}$, for the ſecond year by the ſeries $\frac{S.b''.\overline{s-t}.\overline{d-e}}{6abcr^2}+\frac{S.b''.\overline{s-t}.e}{2abcr^2}+\frac{S.b''\overline{a-s}.e}{abcr^2}+\frac{S.b''.\overline{a-s}.\overline{d-e}}{2abcr^2}$, for the third year by the ſeries $\frac{S.b'''.\overline{t-u}.\overline{e-f}}{6abcr^3}+\frac{S.b'''.\overline{t-u}.f}{2abcr^3}+\frac{S.b'''.\overline{a-t}.f}{abcr^3}+\frac{S.b'''.\overline{a-t}.\overline{e-f}}{2abcr^3}$, and ſo on for the remaining years of A's life. Theſe ſeveral ſeries may be found $=\frac{S}{abcr}\times\overline{-\frac{ab'c}{3}-\frac{b'cs}{6}-\frac{ab'd}{6}-\frac{b'ds}{3}+}$

$ab'd$

$\overline{\frac{ab'd}{2}+\frac{ab'c}{2}}+\frac{S}{abcr^2}\times\overline{-\frac{sb''d}{3}-\frac{b''dt}{6}-\frac{sb''e}{6}-\frac{b''et}{3}+\frac{ab''e}{2}+\frac{ab''d}{2}}+\frac{S}{abcr^3}\times$ $\overline{-\frac{tb'''e}{3}-\frac{b'''eu}{6}-\frac{tb'''f}{6}-\frac{b'''fu}{3}+\frac{ab'''f}{2}+\frac{ab'''e}{2}}$, &c. &c. Let x repreſent the number of perſons living at the age of H, a perſon one year younger than A; let N denote a perſon one year older than A, and let the ſeveral combinations BN, BNC, AB, &c. denote, as in the former caſe, the values of annuities on the joint lives of B and N, of B, N and C, of A and B, &c.; then by proceeding in the ſame manner as in the foregoing demonſtration the ſeries $\frac{ab'c}{3abcr}+\frac{sb''d}{3abcr^2}+\frac{tb'''e}{3abcr^3}+$&c. may be found $=\frac{ax}{3ac}\times\overline{HK-HBK}-\frac{AC-ABC}{3r}$; the ſeries $\frac{b'cs}{6abcr}+\frac{b''dt}{6abcr^2}+\frac{b'''eu}{6abcr^3}+$&c. $=\frac{x}{6c}\times\overline{AK-ABK}-\frac{s}{6ar}\times\overline{NC-NBC}$; the ſeries $\frac{ab'd}{6abcr}+\frac{sb''e}{6abcr^2}+\frac{tb'''f}{6abcr^3}+$&c. $=\frac{x}{6a}\times\overline{HC-HBC}-\frac{d}{6cr}\times\overline{AT-ABT}$; and the ſeries $\frac{b'ds}{3abcr}+\frac{b''et}{3abcr^2}+\frac{b'''fu}{3abcr^3}+$&c. $=\frac{AC-ABC}{3}-\frac{sd}{3acr}\times\overline{NT-NTB}$. Theſe four ſeries, therefore, ſuppoſing them all to be poſitive quantities are $=\frac{x}{3r}\times\overline{\frac{x.\overline{HK-HBK}}{a}+\frac{AK-ABK}{2}}+\frac{x}{6a}\times\overline{HC-HBC}+\frac{\overline{r-1}}{3r}\times\overline{AC-ABC}-\frac{s}{6ar}\times\overline{NC-NBC}-\frac{d}{3cr}\times\overline{\frac{AT-ABT}{2}+\frac{s.\overline{NT-NBT}}{a}}$. With reſpect to the two remaining ſeries $\frac{b'd}{2bcr}+\frac{b''e}{2bcr^2}+\frac{b'''f}{2bcr^3}+$&c. . . . and $\frac{b'c}{2bcr}+\frac{b''d}{2bcr^2}+\frac{b'''e}{2bcr^3}+$&c., theſe, it is evident, are to be continued after the deceaſe of A till the extinction of the joint lives of B and C, and have been already proved in the ſolution of the ſecond problem in my former Paper, to denote the value of the given ſum payable if C ſhould ſurvive B. Let this value be repreſented by R and the ſum of the foregoing expreſſions (or $\frac{x}{3c} \times \overline{\frac{a\,.\,\overline{HK-HBK}}{a} + \frac{AK-ABK}{2}} + \frac{a}{6a} \times \overline{HC-HBC}$, &c.) by M, then will the value of the ſum S (when A is the oldeſt of the three lives) be $= S \times \overline{R-M}$. Q. E. D.

If, the three lives be equal, the value of the given ſum for the firſt year will be $= \frac{S\,.\,\overline{c-d}|^3}{6\,.\,c^3\,.\,r} + \frac{S\,.\,\overline{c-d}|^2\,.\,d}{2c^3\,.\,r} = S \times \overline{\frac{1}{6r} + \frac{2a^3}{6c^3r} - \frac{3dd}{6c^2r}}$; the value of the ſame ſum for the ſecond year will be $= \frac{S\,.\,\overline{d-e}|^3}{6c^3r^2} + \frac{S\,.\,\overline{d-e}|^2\,.\,e}{2c^3r^2} + \frac{S\,.\,\overline{d-e}\,.\,\overline{c-d}\,.\,e}{c^3r^2} + \frac{\overline{d-e}|^2\,.\,\overline{c-d}}{2c^3r^2} = S \times \overline{\frac{2e^3}{6c^3r^2} - \frac{3ee}{6c^2r^2} - \frac{2d^3}{6c^3r^2} + \frac{3dd}{6c^2r^2}}$; the value for the third year will be $= S \times \overline{\frac{2f^3}{6c^3r^3} - \frac{3ff}{6c^2r^3} - \frac{2e^3}{6c^3r^3} + \frac{3ee}{6c^2r^3}}$, and ſo on for the other years to the extremity of life. Let CC and CCC denote the values of the two equal and three equal joint lives, the ſum of theſe ſeries may then be found $= \frac{S}{6} \times \overline{\frac{1}{r} + \overline{2CCC - 3CC} + \frac{3\,.\,\overline{CC-2CCC}}{r}}$ $=$ (ſuppoſing the perpetuity, or $\frac{1}{r-1}$, to be denoted by V) $\frac{S}{6} \times \overline{\frac{r-1}{r}} \times \overline{V - 3\,.\,\overline{CC-2CCC}}$.

It muſt be here remembered, that from other principles it is well known, that the number of years purchaſe expreſſing the value of an *eſtate* or *perpetual annuity* to be entered upon at the failure of two out of any three equal lives is, "the difference "between three times the values of two equal joint lives, and "twice the values of three equal joint lives ſubtracted from "the perpetuity," or $V - \overline{3CC - 2CCC}$. The value, therefore, of ſuch a reverſion, ſuppoſing it to depend on the failure of the three equal lives in any one particular order, is (ſince

there

there are ſix ſuch orders equally probable) $\frac{1}{6}\times\overline{V-3CC-2CCC}$. But it appears, from the correction explained in Dr. PRICE's Treatiſe on Reverſionary Payments, Vol. I. p. 34. that the value of a reverſionary *ſum* is always leſs than the value of an equivalent reverſionary *eſtate* in the proportion of 1 to r. The ſum being S the equivalent eſtate or perpetual annuity is always $S\times\overline{r-1}$; and conſequently the value of the ſum S depending on the ceaſing of three equal lives in any one particular order and thus determined, is the ſame with that determined by the foregoing inveſtigation, that is, $\frac{S}{6}\times\frac{\overline{r-1}}{r}\times\overline{V-3CC-2CC}$. The inveſtigation, therefore, is right, and the correction and inveſtigation demonſtrate one another.

But the foregoing expreſſion for determining the value of the reverſion in this particular caſe is not only obtained immediately from the ſeries, but alſo from the two different rules which have been given for determining the value when the lives are unequal; and hence a proof ariſes of the truth of theſe rules, as well as of the reaſoning upon which they are founded. Thus the firſt rule, ſuppoſing the lives all equal, becomes $\frac{x^2}{c^2}\times\frac{KK-CKK}{6}-\frac{dd}{cc.r}\times\frac{TT-CTT}{6}-\frac{\overline{r-1}}{r}\times\frac{CC-CCC}{3}+\frac{x}{c}\times\frac{CK-CCK}{6}-\frac{d}{cr}\times\frac{CT-CCT}{6}$, and the ſecond rule becomes $\frac{\overline{V-CC}.\overline{r-1}}{2r}-\frac{x^2}{c^2}\times\frac{KK-CKK}{3}+\frac{dd}{cc.r}\times\frac{TT-CTT}{3}-\frac{r-1}{r}\times\frac{CC-CCC}{3}-\frac{x}{c}\times\frac{CK-CCK}{3}+\frac{d}{cr}\times\frac{CT-CCT}{3}$. Let the value according to the firſt rule be denoted by L, and the ſecond rule will be $=\frac{\overline{r-1}.\overline{V-CC}}{2r}-2L-\frac{\overline{r-1}.\overline{CC-CCC}}{r}(=L)$. Hence $3L=$

$$\frac{\overline{r-1}.\overline{V\quad CC}-2.\overline{r\quad 1}.\overline{CC-CCC}}{2r} \text{ and } L=\frac{\overline{r-1}}{6r}\times\overline{V-3CC-2CCC}.$$

Q. E. D.

Were we poſſeſſed of complete tables of the values of annuities on two and three joint lives, the preceding rules would give an eaſy and exact ſolution of this problem in all caſes. But as ſuch tables, computed for every age, would be a work of immenſe difficulty, eſpecially in regard to the values of three joint lives, Mr. SIMPSON's rule for approximating to theſe from the given values of the two joint lives, has hitherto been adopted, and it ſeems upon the whole to anſwer the purpoſe very well. In the preſent problem it is attended with no other inconvenience than increaſing the labour of the computations; for the values of the reverſions derived from it appear in general to be perfectly correct. This is more fully aſcertained by a table which Dr. PRICE has given in his Treatiſe on Reverſionary Payments (Vol. II. Table 37.), of the values of three equal joint lives computed at 4 *per cent.* from the probabilities of life at NORTHAMPTON. By the aſſiſtance of this table, when the lives are of the ſame age, it is evident, from what has been already obſerved, that the exact value of the reverſion may be eaſily obtained. The few following ſpecimens computed from it, and compared with the values of the reverſions deduced from the firſt and ſecond of the preceding rules, demonſtrate the accuracy of thoſe rules: for, notwithſtanding the approximated values of the three joints lives have been uſed in every inſtance in which the rules have been employed, yet the reſults approach ſo near the truth, even in the laſt ſtages of life, when the decrements are moſt irregular, that, though derived from theſe approximations, there can be little doubt of their correctneſs in almoſt every other period of life.

Common age.	Exact value * of £. 100. computed from Dr. PRICE's Tables of the values of two and three equal joint lives.	Value of £. 100. computed from the firſt of the foregoing rules, and from Mr. SIMPSON's approximation to the values of three joint lives.	Value of £. 100. computed from the ſecond of the foregoing rules, and from Mr. SIMPSON's approximation to the values of three joint lives.
70	12.000	12.005	12.000
75	12.944	12.943	12.943
80	13.840	13.810	13.880
85	14.450 †	14.670	14.340

Mr. DODSON ‡, and Mr. SIMPSON §, are the only writers who have ſolved, or rather who have approximated to the ſolution of this problem. But the former, by deducing his rules immediately from a wrong hypotheſis, having rendered

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* That is, *one-ſixth* part of the whole reverſion.

† The ſeveral reverſions in this column, when computed from SIMPSON's approximation to the values of the three joint lives, are 12.012, 12.933, 13.847, and 14.803 reſpectively; which upon the whole differing nearly as much from the real values as thoſe in the two other columns afford a convincing proof, that the very ſmall deviation from the truth in theſe latter values proceeds not from any inaccuracy in the rules themſelves, but ſolely from having uſed the *approximated* inſtead of the *real* values of the three joint lives. And this alſo will account for the difference in the values by the firſt and ſecond rules. Were thoſe values computed from tables which give the correct values of two and three joint lives at all ages, they would come out exactly the ſame. In the two firſt examples, where the values by one rule are true, it appears, that the values by the other rule are equally ſo. In the two laſt examples, where the values are not quite ſo accurate, it may be obſerved, that they differ as much in exceſs by one rule as they do in defect by the other; which muſt in general be the caſe from the very nature of thoſe rules; for if L (or the value by the firſt rule) be greater than the truth, the difference between $\frac{\overline{r-1}\,.\,\overline{V-CC}}{2r}$ and 2L (or the value by the ſecond rule) muſt be leſs than the truth; and, on the contrary, if L be leſs, this difference will be greater than the truth.

‡ See DODSON's Mathematical Repoſitory, Vol. III. Queſtions 42, 43, &c.

§ See SIMPSON's Select Exerciſes, Prob. 38.

moſt

moſt of them (eſpecially thoſe in which three lives are concerned) of no uſe, it will be unneceſſary to take notice of what he has done on the ſubject. With regard to the latter, whoſe rule is not only the ſole guide for determining the value of this reverſion, but alſo the ſource from which a great variety of other problems are ſolved, perhaps it may not be improper to examine how far his ſolution is to be depended upon; and the following examples have therefore been computed for this purpoſe.

TABLE I.

Ages of C.	B.	A.	Value, by SIMPSON's rule, of £. 100. payable on the contingent reverſion ſpecified in this problem, when either C or B are eldeſt, according to the *Northampton* Table and at 4 *per cent.*	True value of the ſame reverſion computed from the firſt rule in the foregoing ſolution.
80	70	40	1.926	1.179
75	65	25	1.873	1.032
65	50	15	2.090	1.690
70	80	40	6.615	6.117
50	65	15	5.580	3.879
78	78	20	2.583	1.982
45	60	12	5.571	4.133
60	45	12	2.292	1.686

TABLE II.

Ages. C.	B.	A.	Value of the ſame reverſion by SIMPSON's rule, when A is the oldeſt of the three lives.	True value of the ſame reverſion by the ſecond rule in the foregoing ſolution.
24	65	75	34.636	31.792
65	24	75	6.305	7.895
49	9	69	7.351	5.960
18	78	78	37.554	33.019

TABLE

TABLE III.

Common Age.	Value of the ſame reverſion by SIMPSON's rule, when the ages of the three lives are equal.	True value of the ſame reverſion.
70	13.20	12.00
75	14.98	12.94
80	16.58	13.84
85	17.86	14.45

By comparing the values in the preceding tables, Mr. SIMPSON's rule appears in almoſt every inſtance to be exceedingly incorrect. Even when the lives are equal (in which caſe it might have been expected to be ſufficiently accurate) it ſeems to deviate, in old age at leaſt, ſo widely from the truth as to be unfit for uſe. When C or B are eldeſt (which, however, is a caſe that does not often occur), the reſults ſometimes exceed the truth *one-half*, and generally by more than *one-third* of the real value. When A is the oldeſt of the three lives (which is the moſt common caſe) theſe reſults are erroneous in nearly an equal degree. Nay, in ſome caſes, Mr. SIMPSON's rule is not only wrong but abſurd. Thus, in the laſt example in the ſecond table, the value of £. 100. payable on the contingency of C aged 18 ſurviving B aged 78 after A aged 78, is by this rule = £. 37.554. The value, therefore, of the ſame ſum on the contingency of C's ſurviving A after B is alſo £. 37.554. Hence the value of £. 100. on the contingency of C's ſurviving A and B (without the reſtriction of one dying before the other) is 2 × 37.554 = £. 75.108 *. By another rule of Mr. SIMPSON †, the value of £. 100, on the contingency of C's ſurviving B only, is no more than £. 74 *. Now it is ſelf-evident, that this latter value, inſtead of being *leſs*, ought to have been *greater* than the former, inaſmuch as the probability of C's ſurviving only one life muſt be greater than that of his ſurviving two lives.

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* See SIMPSON's Select Exerciſes, Prob. 39.   † Ibid. Prob. 32.

Many additional inſtances might be produced in which this rule, being made the baſis upon which the ſolutions of other problems are founded, leads to concluſions equally erroneous. But theſe enquiries would be improper here; and I ſhall only obſerve, that had the foregoing examples been computed from the SWEDEN or LONDON, inſtead of the NORTHAMPTON Table, this rule would have appeared to be ſtill more incorrect than it does from thoſe computations.

When Mr. SIMPSON wrote his Select Exerciſes, he was in a great meaſure obliged to have recourſe to DE MOIVRE's hypotheſis, for want of thoſe excellent tables of the real probabilities of life, and alſo of the values of ſingle and joint lives which have been ſince publiſhed. Had he been poſſeſſed of theſe, it is moſt likely that his ſuperior abilities would have directed him to a more accurate method of inveſtigation. At preſent there can be no juſt reaſon for ever recurring to this wretched hypotheſis. The ſolutions of all caſes of two and even of three lives may be effected without much difficulty from principles ſtrictly true. But I muſt here take my leave of this ſubject, hoping that its importance may engage other mathematicians to the further proſecution of it.

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* The true values are £. 66.038. and £. 74.884. reſpectively.

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VI. *Reſult of Calculations of the Obſervations made at various Places of the Eclipſe of the Sun, which happened on* June 3, 1788. *By the Rev.* Joſeph Piazzi, *C. R. Profeſſor of Aſtronomy in the Univerſity of* Palermo; *communicated by* Nevil Maſkelyne, *D. D. F. R. S. and Aſtronomer Royal.*

Read January 15, 1789.

TO DR. MASKELYNE.

SIR,

THE ſatisfaction I had in obſerving the eclipſe of the ſun on the third of June laſt, with you and M. D'ARQUIER, at Greenwich, induces me to give you an account of the uſe made of the obſervations, and the conſequences I have drawn from them. The obſervations which I have collected concerning the ſame eclipſe, and which were made in other places, contribute to the extenſiveneſs of my calculations, and to determine the poſition of certain places, which had not been before accurately determined, as that of Dublin, that of Mitau in Courland, and Perinaldo in Italy. The longitudes of all the other places muſt be referred to that of the Royal Obſervatory at Greenwich, as being the firſt in Europe, and becauſe the obſervations which you have made in it, are by far more accurate than any others made elſewhere.

The reſult of the obſervation at Greenwich is confirmed in the moſt ſatisfactory manner by the obſervations of Oxford and Loampit-Hill, which Dr. HORNSBY and Mr. AUBERT have done me the honour to communicate to me. Theſe three obſervations perfectly agree in the latitude of the moon; whence it follows, that the duration of the eclipſe was juſtly obſerved. And whereas the difference of longitude for Oxford, as determined by theſe obſervations, is only one ſecond, and that of Loampit-Hill only two ſeconds, different from that which had been determined by means of the beſt time-keepers and other moſt exact obſervations, it follows, that theſe three obſervations may be conſidered as a ſingle one, having a treble degree of accuracy. In effect, if the moment of conjunction found for Oxford and Loampit-Hill be reduced to the meridian of Greenwich, by adding to the latter 5″,4, and to the former 5′ 0″, and a mean be taken, it will appear, that this mean differs only by 0,6 of a ſecond from the conjunction deduced from the obſervation made at Greenwich only. This alſo clearly proves, that the eclipſes of the ſun, when accurately obſerved, give nearly the ſame exactneſs as the occultations of the ſtars, which from their nature are conſidered as the moſt exact.

The obſervation made at Dublin deſerves our greateſt attention, particularly ſince the eſtabliſhment of a very excellent obſervatory there. Dr. USSHER confeſſes, that the longitude of that city has not been exactly determined (ſee the Tranſactions of the Royal Iriſh Academy for the year 1787, p. 86.). He ſuppoſes the longitude of Dublin to be 24′ 58″ W. which he determined by means of a time-keeper, which Mr. ARNOLD happened to take with him to that city; whereas I find it to be 25′ 13″,4. This my determination I believe not to leave the uncertainty of two ſeconds;

ſeconds, becauſe the latitude of the moon deduced from the obſervation is the ſame with that of Greenwich.

The obſervation of Mitau is likewiſe very intereſting; it ſhews the ſituation of a diſtant country, where no obſervation had been made before M. BEITLER eſtabliſhed an obſervatory there. The obſervation of this able aſtronomer is of ſuch correctneſs, that it furniſhes the ſame latitude of the moon as the preceding ones; conſequently the difference of the meridians, which I thence deduce, may be conſidered to be as exact as that of Dublin. The difference is 1 h. 34′ 54″ E, which becomes of the greater importance to geography, becauſe from Pomerania to Peterſburg no one point had been accurately determined before.

The poſition of Berlin has been already determined by means of ſome eclipſes; but the reſults do not agree. The difference of 53′ 32″, which I have deduced, not only agrees exactly with that mentioned in Vol. IV. of M. DE LA LANDE's Aſtronomy, and which this famous aſtronomer had deduced from the occultation of Antares obſerved by himſelf in the year 1749; but it alſo comes ſo near to the longitude mentioned by Meſſ. LEXELL and BERNOULLI, in the Ephemeris of Berlin, as not to differ by more than two ſeconds.

The obſervation of Vienna gives for the difference of the meridians 1 h. 5′ 31″. Though this determination differs but 1″ from that found in the Almanack of Milan, and in the *Requiſite Tables*, yet the obſervations of the two phaſes had not been very accurately made.

Perinaldo in Italy is a place whoſe poſition has not been as yet well determined. The tables requiſite for the Nautical Ephemeris lay down this place at 30′ 40″ to the Eaſt of Greenwich; ſome place it at 30′ 20″. The obſervation made by

M. MARALDI, nephew of the Paris Academician, which I have by me, gives 30′ 53″ E. for the difference of the meridians, and this may be conſidered as the beſt hitherto known.

The obſervations made at Milan, by the aſtronomers DE CESARIS and REGGIO, were interrupted by intervening clouds. In fact, the latitude of the moon in conjunction comes out only equal to 14′ 32″, which ſhews that the duration of the eclipſe was not properly obſerved. I have thence alſo calculated the conjunction ſeparately for the beginning and for the end of the eclipſe, and I have found out the following differences of meridians, *viz.* for the beginning 36′ 39″,6, and for the end 36′ 38″; and for the end and beginning conjointly 36′ 37″. This laſt difference comes neareſt to that mentioned in the Milan Ephemeris for the year 1789, which is 36′ 41″. The obſervation made at Bologna aſſigns 45′ 28″ E. for the difference of the meridians. But the duration of the eclipſe was not properly obſerved. However, notwithſtanding this imperfection, it may happen that the reſult determined is exact.

The two obſervations of France, *viz.* that of Viviers, and that of Rouen, give almoſt the ſame difference which I find in the Requiſite Tables; that of Rouen differing only 1″, and that of Viviers 2″. As the difference of the meridians between Paris and Rouen is known with the greateſt preciſion to be 4′ 57″ to the W. of Paris; if to this difference are added 4′ 22″,3, which is the difference I found between Rouen and Greenwich, there will reſult, for the difference of the meridians between the Obſervatory of Greenwich and Paris, 9′ 19″,3. This difference only differs by 0″,7 from that eſtabliſhed by Dr. BRADLEY, which is 9′ 20″, as adopted by yourſelf, and lately confirmed by Major-general ROY.

The obſervations of Loampit-Hill, Greenwich, and Oxford, as they ſerve for the baſis of all my calculations, I have calculated them two different ways, *viz.* by the method of parallactic angles, and by the method of the nonageſimal, and the reſults agreed together within a few tenths of a ſecond. By theſe two different methods I have alſo calculated the obſervations of Vienna, Berlin, and Viviers, in order to ſhew, that the different latitudes of the moon, given by the various obſervations, were not owing to any error in my calculations. For theſe places, in which both the beginning and the end of the eclipſe had been obſerved, I have deduced the time of conjunction from the two phaſes conjointly, which have alſo given the duration of the eclipſe, which cannot be obtained from a ſingle obſervation.

The error of the tables which reſults from the obſervation at Greenwich is +26″ in longitude, and +11″,5 in latitude, at 20 h. 58′ 47″,3 of apparent time, taking for the longitude of the ſun 2 s. 14° 16′ 54″,7, as I deduce from the Nautical Almanack, and that of the moon at the ſame time to be greater than the ſun by 26″, as deduced from the ſame Almanack. I ſuppoſe alſo the horary motion of the moon in the ecliptic, by taking it an half hour before and after the conjunction, to be 36′ 52″+0″,6 for the hour following the conjunction, and −0″,6 for the hour preceding the conjunction; the moon's horary motion in latitude is 3′ 24″,3; the horizontal parallax of the moon *minus* that of the ſun at Greenwich, to be 60′ 14″,4 for the commencement of the eclipſe, and 60′ 16″,4 for the end; the ſun's diameter 31′ 34″,6, leſs by 3″ than that given in the Almanack, according to the correction which you have found neceſſary to be made; the moon's diameter I have ſtated as in the Almanack. In the opinion of M. DE LA LANDE, ſome correction ought to be made to the parallax and to the diameter of the moon, as well as to the diameter of the ſun; but on the one hand this would not make any alteration in the difference of the meridians which I have found; and on the other I thought proper to make uſe of thoſe elements the Nautical Almanack furniſhed me with, that being a work the moſt perfect of the kind that ever appeared, and to which all aſtronomers and navigators ought to pay the greateſt attention.

In fine, I compared the moon's longitude in conjunction, deduced from the eclipſe with the new tables of the moon corrected by Mr. Mason, and found the longitude by thoſe tables to be 2 s. 14° 17′ 6″,4, and the latitude to be 15′ 1″,3. The error then of the new Tables is + 11″,7 in longitude, and + 13″,1 in latitude; but M. de la Lande having lately ſent to me from Paris the place of the ſun, calculated with the new Solar Tables (a moſt uſeful improvement which M. de Lambre has, with much ingenuity, deduced from your obſervations) I find the error in longitude to be + 27″,4, the ſun's place being 2 s. 14° 16′ 39″,0 at 20 h. 58′ 47″,3.

The following table contains the obſervations of the eclipſe, and the reſults deduced from thence. The firſt vertical column ſhews the name and place of the obſervers; the two next vertical columns contain all thoſe obſervations which have been made, in apparent time; the other columns ſhew the reſults, *viz*. the fourth column, contains the true conjunction in apparent time; the fifth column contains the longitude of the moon in conjunction, which being always the ſame, needs not to be repeated under every perpendicular column; the ſixth column contains the latitude of the moon, which, as it depends upon the manner of obſerving the two phaſes, is ſubject to ſome variety; the ſeventh or laſt column contains the difference between the various meridians and that of Greenwich.

This, Sir, in brief, is the reſult which I have been able to deduce from the various obſervations above mentioned, and which I intirely ſubmit to your judgement. If you think that it deſerves to be made public, and in that caſe would be pleaſed to preſent this Paper to the Royal Society, I ſhall eſteem myſelf extremely honoured and obliged by it.

I have the honour to be, &c.

JOSEPH PIAZZI.

Table of the obſervations made at various places on the eclipſe of the ſun, which happened June 3, 1788, and of reſults deduced from the ſame.

	Beginning.	End.	Conjunction.	Longitude of the moon in conjunction.	Latitude in conjunction	Difference of meridians.
Greenwich, Dr. MASKELYNE.	h. ′ ″ 19 24 46,5	h. ′ ″ 21 1 24,0	h. ′ ″ 20 58 47,3		′ ″ 14 48,2	0
Loampit-Hill, Mr. AUBERT.	19 24 41,9	21 1 20,3	20 58 44,1		14 48,2	″ 3,2W
Oxford, Dr. HORNSBY.	19 20 36,1	20 54 40,0	20 53 46,2		14 48,7	′ ″ 5 1,1W
Dublin, Dr. USSHER.	19 5 46,5	20 27 42,1	20 33 33,9		14 48,3	25 13,4W
Mittau, M. BEITLER.	21 20 15,0	23 8 52,0	22 33 41,5		14 48,7	h. ′ ″ 1 34 54,2E
Berlin, M. BODE.	20 23 9,0	22 14 32,0	21 52 20,3		14 44,2	0 53 33E
Vienna, M. TRIESNEKER.	20 25 49,0	22 32 40,0	22 4 18,8	S. ° ′ ″ 2 14 16 54,7	14 39,0	1 5 31,5E
Viviers, M. FLAUGERGUAS.	19 26 38,0	21 25 41,0	21 17 29,0		14 33,0	′ ″ 18 41,7E
Perinaldo, M. MARALDI.	19 37 50,0	*	21 29 40		*	30 53,0E
Rouen, M. DU LAGNE.	*	21 7 15,0	21 3 9,6		*	4 22,3E
Milan, Meſſ. DE CESARIS and REGGIO.	19 48 23,0	21 51 14,0	21 35 24,7		14 32,0	36 37,4E
Bologna, M. MATTEUCCI.	19 55 10,5	22 3 45,5	21 44 15,3		14 31,0	45 28E
Padua, M. CHIMINELLO.	19 59 20,0	22 6 58,0	21 46 21,3		14 39,0	47 34E

POSTSCRIPT.

IN the month of February laſt, I was favoured by Count **DE BRUHL** with the obſervation of the eclipſe of the 4th of June laſt, made at Warſaw by M. **BYSTRZYSKI**; about the ſame time I alſo received of M. **DE LA LANDE** ſome other obſervations of the ſame eclipſe, *viz.* thoſe made at Prague, Marſeilles, Creſmunſter, and Bagdad in Meſopotamia, which I immediately calculated, in order to add them to the others, which Dr. **MASKELYNE** lately did me the honour of preſenting to the Royal Society.

The obſervation of Marſeilles confirms in the beſt manner the difference of meridians ſet down in the *Requiſite Tables*, differing from that only by a ſecond. The obſervation of Warſaw gives a difference ten ſeconds greater, and that of Creſmunſter fourteen ſeconds leſs; which differences ought not to ſurprize us, conſidering the obſervations upon which the longitudes of theſe two places had been eſtabliſhed; but, on the other hand, the obſervation of Prague clearly proves, that the ſituation of that town had been much leſs accurately determined than one might have expected. The time for the conjunction, which reſults from this obſervation, is the very ſame as that which is deduced by M. **GERSTNER**'s new method, deſcribed in the Berlin Ephemeris for the year 1791, p. 243. From this time of conjunction the difference of meridians comes out equal to 57′ 42″,7, *viz.* one minute and ſeventeen ſeconds leſs that that of the Requiſite Tables.

The calculation of the obſervation made at Bagdat ſeems to indicate that there is ſome miſtake with regard to the end of the eclipſe, having found, that the difference of apparent longitude at the end is 20″ greater than the ſum of the ſemidiameters of the ſun and moon, increaſed in proportion of the apparent altitude of the moon: for this reaſon I do not give the moon's latitude in conjunction. As for the time of the conjunction, I deduce it both from the two phaſes together, and from the commencement only, having previouſly corrected the moon's latitude of the error which I diſcovered in the tables, *viz.* 11″,6. The time of conjunction which reſults from the firſt calculation is 23 h. 56′ 11″; that which reſults from the ſecond 23 h. 56′ 16″: this laſt nearly agrees in the difference of meridians with the Ephemeris of Paris for the year 1789, and differs from the Requiſite Tables by 2′ 32″.

The following table repreſents, as the firſt, the obſervations and the reſults.

	Beginning.	End.	Conjunction.	Longitude.	Latitude.	Difference of meridians.
	h. ′ ″	h. ′ ″	h. ′ ″		′ ″	h. ′ ″
Warſaw, M. BYSTRZYSKI	20 56 45	22 57 33	22 22 59,3		14 44	1 24 12
Prague, M. STRNADT.	20 21 29	22 21 15	21 56 30		14 45	0 57 42,7
Marſeilles, M. BERNARD.	19 26 42	21 29 23,5	21 20 17,5		14 40	0 21 30,2
Creſmunſter, M. FIXLMILLNER.	20 15 20	22 19 50,7	21 54 59		14 23	0 56 11,7
Bagdad, M. DE BEAUCHAMP.	22 30 51	23 26 19	23 56 11		*	2 57 23,7

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VII. *An Account of a bituminous Lake or Plain in the Iſland of* Trinidad. *By Mr.* Alexander Anderſon; *communicated by Sir* Joſeph Banks, *Bart. P. R. S.*

Read February 19, 1789.

A MOST remarkable production of nature in the iſland of Trinidad, is a bituminous lake, or rather plain, known by the name of Tar Lake; by the French called La Bray, from the reſemblance to, and anſwering the intention of, ſhip pitch. It lies in the leeward ſide of the iſland, about half-way from the Bocas to the ſouth end, where the Mangrove ſwamps are interrupted by the ſand-banks and hills; and on a point of land which extends into the ſea about two miles, exactly oppoſite to the high mountains of Paria, on the north ſide of the Gulf.

This cape, or head-land, is about fifty feet above the level of the ſea, and is the greateſt elevation of land on this ſide of the iſland. From the ſea it appears a maſs of black vitrified rocks; but, on a cloſe examination, it is found a compoſition of bituminous ſcoriæ, vitrified ſand, and earth, cemented together; in ſome parts beds of cinders only are found. In approaching this Cape, there is a ſtrong ſulphureous ſmell, ſometimes diſagreeable. This ſmell is prevalent in many parts of the ground to the diſtance of eight or ten miles from it.

This point of land is about two miles broad, and on the eaſt and weſt ſides, from the diſtance of about half a mile from the ſea, falls with a gentle declivity to it, and is joined to the main land on the ſouth by the continuation of the Mangrove ſwamps; ſo that the bituminous plain is on the higheſt part of it, and only ſeparated from the ſea by a margin of wood which ſurrounds it, and prevents a diſtant proſpect of it. Its ſituation is ſimilar to a Savannah, and, like them, it is not ſeen till treading upon its verge. Its colour, and even ſurface, preſent at firſt the aſpect of a lake of water; but I imagine it got the appellation of Lake when ſeen in the hot and dry weather, at which time its ſurface to the depth of an inch is liquid, and then from its coheſive quality it cannot be walked upon.

It is of a circular form, and I ſuppoſe about three miles in circumference. At my firſt approach it appeared a plane, as ſmooth as glaſs, excepting ſome ſmall clumps of ſhrubs and dwarf-trees that had taken poſſeſſion of ſome ſpots of it; but when I had proceeded ſome yards on it, I found it divided into areolæ of different ſizes and ſhapes: the chaſms or diviſions anaſtomoſed through every part of it; the ſurface of the areolæ perfectly horizontal and ſmooth; the margins undulated, each undulation enlarged to the bottom till they join the oppoſite. On the ſurface the margin or firſt undulation is diſtant from the oppoſite from four to ſix feet, and the ſame depth before they coaleſce; but where the angles of the areolæ oppoſe, the chaſms or ramifications are wider and deeper. When I was at it, all theſe chaſms were full of water, the whole forming one true horizontal plane, which rendered my inveſtigation of it difficult and tedious, being neceſſitated to plunge into the water a great depth in paſſing from one areola to another. The trueſt idea that can be formed of its ſurface will be from the areolæ and their ramifications on the back of a turtle. Its more common conſiſtence and appearance is that of pit-coal, the colour rather greyer. It breaks into ſmall fragments, of a cellular appearance and gloſſy, with a number of minute and ſhining particles interſperſed through its ſubſtance; it is very friable, and, when liquid, is of a jet black colour. Some parts of the ſurface are covered with a thin and brittle ſcoria, a little elevated.

As to its depth, I can form no idea of it; for in no part could I find a ſubſtratum of any other ſubſtance; in ſome parts I found calcined earth mixed with it.

Although I ſmelt ſulphur very ſtrong on paſſing over many parts of it, I could diſcover no appearance of it, or any rent or crack through which the ſteams might iſſue; probably it was from ſome parts of the adjacent woods: for although ſulphur is the baſis of this bituminous matter, yet the ſmells are very different, and eaſily diſtinguiſhed, for its ſmell comes the neareſt to that of pitch of any thing I know. I could make no impreſſion on its ſurface without an axe: at the depth of a foot I found it a little ſofter, with an oily appearance, in ſmall cells. A little of it held to a burning candle makes a hiſſing or cracking noiſe like nitre, emitting ſmall ſparks with a vivid flame, which extinguiſhes the moment the candle is removed. A piece put in the fire will boil up a long time without ſuffering much diminution: after a long time's ſevere heat, the ſurface will burn and form a thin ſcoria, under which the reſt remains liquid. Heat ſeems not to render it fluid, or occupy a larger ſpace than when cold; from which, I imagine, there is but little alteration on it during the dry months, as the ſolar rays cannot exert their force above an inch below the ſurface. I was told by one Frenchman, that in the dry ſeaſon the whole was an uniform ſmooth maſs; and by another, that the ravins contained water fit for uſe during the year; but neither can I believe: for if, according to the firſt aſſertion, it was an homogeneous maſs, ſomething more than an external cauſe muſt affect it, to give it the preſent appearances: nor without ſome hidden cauſe can the ſecond be granted. Although the bottoms of theſe ramified channels admit not of abſorption, yet from their open expoſure, and the black ſurface of the circumjacent parts, evaporation muſt go on amazing quick, and a ſhort time of dry weather muſt ſoon empty them; nor from the ſituation and ſtructure of the place is there a poſſibility of ſupply, but from the clouds. To ſhew that the progreſs of evaporation is inconceivably quick here, at the time I viſited it, there were, on an average, two-thirds of the time inceſſant torrents of rain; but from the afternoon being dry, with a gentle breeze (as is generally the caſe during the rainy ſeaſon in this iſland), there evidently was an equilibrium between the rain and the evaporation; for in the courſe of three days I ſaw it twice, and perceived no alteration on the height of the water, nor any outlet for it but by evaporation.

I take this bituminous ſubſtance to be the *bitumen aſphaltum* Linnæi. A gentle heat renders it ductile; hence, mixed with a little greaſe or common pitch, it is much uſed for the bottoms of ſhips, and for which intention it is collected by many, and I ſhould conceive it a preſervative againſt the Borer, ſo deſtructive to ſhips in this part of the world.

Beſides this place, where it is found in this ſolid ſtate, it is found liquid in many parts of the woods; and at the diſtance of twenty miles from this about two inches thick, round holes of three or four inches diameter, and often at cracks or rents. This is conſtantly liquid, and ſmells ſtronger of tar than when indurated, and adheres ſtrongly to any thing it touches; greaſe is the only thing that will diveſt the hands of it.

The ſoil in general, for ſome diſtance round La Bray, is cinders and burnt earths; and where not ſo, it is a ſtrong argillaceous ſoil; the whole exceedingly fertile, which is always the caſe where there are any ſulphureous particles in it. Every part of the country, to the diſtance of thirty miles round, has every appearance of being formed by convulſions of nature from ſubterraneous fires. In ſeveral parts of the woods are hot ſprings; ſome I tried, with a well graduated thermometer of FAHRENHEIT, were 20° and 22° hotter than the atmoſphere at the time of trial. From its poſition to them, this part of the iſland has certainly experienced the effects of the volcanic eruptions, which have heaped up thoſe prodigious maſſes of mountains that terminate the province of Paria on the north; and no doubt there has been, and ſtill probably is, a communication between them. One of theſe mountains oppoſite to La Bray in Trinidad, about thirty miles diſtant, has every appearance of a volcanic mountain: however, the volcanic efforts have been very weak here, as no trace of them extend above two miles from the ſea in this part of the iſland, and the greater part of it has had its origin from a very different cauſe to that of volcanos; but they have certainly laid the foundation of it, as is evident from the high ridge of mountains which ſurrounds its windward ſide to protect it from the depredations of the ocean, and is its only barrier againſt that over-powering element, and may properly be called the ſkeleton of the iſland.

From every examination I have made, I find the whole iſland formed of an argillaceous earth, either in its primitive ſtate, or under its different metamorphoſes. The baſes of the mountains are compoſed of *ſchiſtus argillaceus* and *talcum lithomargo*; but the plains or low-lands remaining nearly in the ſame moiſt ſtate as at its formation, the component particles have not experienced the viciſſitudes of nature ſo much as the more elevated parts, conſequently retain more of their primitive forms and properties. As argillaceous earth is formed from the ſediment of the Ocean, from the ſituation of Trinidad to the Continent, its formation is eaſily accounted for, granting firſt the formation of the ridge of mountains that bound its windward ſide, and the high mountains on the Continent that nearly join it: for the great influx of currents into the Gulf of Paria from the coaſts of Brazil and Andaluſia muſt bring a vaſt quantity of light earthy particles from the mouths of the numerous large rivers which traverſe theſe parts of the Continent; but the currents being repelled by theſe ridges of mountains, eddies and ſmooth water will be produced where they meet and oppoſe, and therefore the earthy particles would ſubſide, and form banks of mud, and by freſh accumulations added would ſoon form dry land; and from theſe cauſes it is evident ſuch a tract of country as Trinidad muſt be formed. But theſe cauſes ſtill exiſt, and the effect from them is evident; for the iſland is daily growing on the leeward ſide, as may be ſeen from the mud-beds that extend a great way into the Gulf, and there conſtantly increaſe. But from the great influx from the Ocean at the ſouth end of the iſland, and its egreſs to the Atlantic again, through the Bocas, a channel muſt ever exiſt between the Continent and Trinidad.

VII. *An Account of a particular Change of Structure in the human Ovarium.* *By* Matthew Baillie, *M. D.*; *communicated by* John Hunter, *Eſq. F. R. S.*

Read February 26, 1789.

THE ovaria in women are ſubject to a great variety of changes from their natural ſtructure. Many of theſe are exactly ſimilar to what take place in other parts of the body; but there is one which ſeems peculiar to them, the nature of which has probably not been hitherto very well aſcertained. The change of ſtructure to which I allude, is a converſion of the natural ſubſtance of an ovarium into a fatty maſs, intermixed with hair and teeth. This ſort of change is rare, although it occurs ſufficiently often to have been ſeen by moſt perſons who are very converſant in the examination of dead bodies. There are many caſes of it related in the different books of diſſections, but, as far as I have diſcovered, moſt commonly without any remarks upon the mode of formation *; or they have been conſidered as very imperfect attempts at the growth of a fœtus in the ovarium, in conſequence of connection between a male and a female. This conjecture reſts no doubt on ſtrong circumſtances of probability, and yet there are many powerful reaſons which ſeem to oppoſe its being well founded. Generation is a proceſs always depending on the action of a certain cauſe, *viz.* the uſual connection between a male and a female; and, when effects ſimilar to thoſe in generation are perceived, it becomes very natural to conclude, that this cauſe has been employed. The bias to ſuch an opinion will become the ſtronger, from reflecting on the paſſions that are known to influence ſo powerfully mankind, by which the agency of this cauſe is frequently excited. When a change, therefore, was obſerved in an ovarium, by which it was converted into a fatty maſs with hair and teeth, this ſhould ſeem to correſpond ſo much with a change taking place in conſequence of generation, that the mind would ſcarcely entertain a doubt of its ariſing from the ſame cauſe, and would readily infer, that it had been preceded by a connection between the ſexes. This doubt would ſtill be the leſs, from the circumſtance of a complete fœtus being ſometimes formed in the ovarium, where the uſual means of generation had been employed. The following caſe, however, exhibits many reaſons why we ſhould be led to believe, that the ovaria in women have ſome power within themſelves of taking on a proceſs which is imitative of generation, without any previous connection with a male; and it is with this view that I proceed to relate it.

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* It has been the opinion of ſome, that hair, teeth, nails, feathers, &c. are animal vegetables or plants; and, agreeably to this opinion, Dr. Tyson conſiders the growth of hair and teeth in the ovarium as a *luſus naturæ*, where nature endeavours to produce ſomething, and being diſappointed in forming an animal, produces a vegetable. Vide Hooke's Lectures and Collection, N° II. p. 11. and 15.

In a female child, about twelve or thirteen years old, which was lately brought to Windmill-ſtreet for diſſection, I found the right ovarium converted into a ſubſtance, doughy to the touch, and about the ſize of a large hen's egg. Upon cutting into the ſubſtance, I found an apparently fatty maſs, intermixed

mixed with hair and an excreſcence of bones. This ſtartled me very much, as I had always been led to believe, that ſuch appearances were a ſort of imperfect conception. The circumſtances altogether being very ſingular, I was led to pay conſiderable attention to the change in the ovarium.

The fatty maſs was of a yellowiſh white colour, in ſome places more yellow than in others, was very unctuous to the feeling, and conſiſted of ſhortened or ſeparated particles, not having the ſame coaleſcence which the fat has generally in the body. It became very ſoft when expoſed to the heat of a fire, and ſunk into a portion of paper, on which it was ſpread, ſo as to make it more tranſparent. When the paper to which it was applied was expoſed to the flame of a candle, it burnt with conſiderable crackling.

The hair with which the fatty ſubſtance was mixed grew out of the inner ſurface of the capſule containing it, in ſome places in ſolitary hairs, but chiefly in ſmall faſciculi, at ſcattered irregular diſtances. Beſides theſe, there were looſe hairs involved in the fatty maſs. The hairs were, ſome of them, of conſiderable length, even to three inches; were fine, and of a light-brown colour. They reſembled much more the hairs of the head, than what are commonly found on the pubis, and correſponded very much in colour to the hair of the girl's head.

There aroſe alſo from the inner ſurface of the capſule ſome veſtiges of human teeth. One appeared to be a canine tooth, another to be a ſmall grinder, two others to be inciſors, and there was alſo a very imperfect attempt at the formation of another tooth. Theſe were not fully formed, the fangs being wanting; but in two of them the bodies were as complete as they are ever found in the common circumſtances. They were each

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of them incloſed in a proper capſule, which aroſe from the inner ſurface of the ovarium, and conſiſted of a white thick opaque membrane. Attached to the capſules of three of the teeth, there was a white ſpungy ſubſtance. The membrane of the ovarium itſelf was of ſome conſiderable thickneſs, but unequal in the different parts, was very ſmooth in its inner ſurface, and more irregular externally. The uterus was ſmaller than it is commonly at birth, was perfectly healthy in its ſtructure, and upon opening into its cavity it exhibited the ordinary appearances of a child's uterus at that period. The left ovarium was very ſmall, correſponding to the ſtate of the uterus. It appears clearly from this, that the uterus had not yet received the increaſe of bulk, which is uſual at the age of puberty. The hymen was entire, ſuch as is commonly found in a child of the ſame age; and there was juſt beginning a *lanugo* upon the labia, not more than what is often found on the upper lip of a boy of fifteen years old. Such are the circumſtances attending this ſingular caſe, and they preſent to the mind various grounds of conſideration.

The formation of hair and teeth is a ſpecies of generation, for in fact it makes a part of it, and ſtrikes the mind as being very different from any irregular ſubſtance which is formed by diſeaſe. This formation too takes place in a part of the body which is ſubſervient to generation, and where a complete fœtus is ſometimes formed. The whole of this looks very much as if the production of hair and teeth in the ovarium was a ſort of imperfect impregnation. But when we take another view of it, there are reaſons at leaſt equally ſtrong for believing that ſuch productions may ariſe from an action in the ovarium itſelf, without any ſtimulus from the application of the male ſemen.

In the caſe before us, the uterus was as ſmall as at birth, indeed more ſo, and the left ovarium (which was perfectly healthy) correſponded to the ſtate of the uterus. It had not been at all ſtimulated, nor did appear capable of being ſtimulated by the application of the male ſemen. This ſeems to be a ſtrong circumſtance; for in a caſe where there was an ovum formed in one of the Fallopian tubes, the uterus was enlarged to more than twice its unimpregnated ſize; and, upon opening into its cavity, the decidua was obſerved to be formed as completely as in the impregnated uterus. This preparation is ſtill preſerved in the collection of Windmill-ſtreet. Nothing can be a ſtronger proof, that when an impregnation takes place out of the cavity of the uterus, the uterus ſtill takes a ſhare in the action, and undergoes ſome of the changes of impregnation. In another preparation, which is preſerved in the ſame collection, where there was a fœtus formed in the ovarium, the uterus was increaſed to more than twice its ordinary ſize, was very thick and ſpungy, and had its blood-veſſels enlarged as in an impregnated uterus. This becomes another very ſtrong proof of the action of the uterus in the formation of an extra-uterine fœtus. In the caſe before us, however, the uterus had undergone no change, and does not ſeem to have arrived at that period, when it could be capable of undergoing ſuch a change.

Beſides, we are not to conſider the formation of teeth in the ovarium to be a quicker proceſs than it is commonly in the head of a fœtus; but in the preſent caſe the teeth having advanced fully as far as they are at ſome months after birth, this proceſs muſt have begun at leaſt more than a twelvemonth before the death of the child. If then we conſider it as an impregnation, ſince the appearances of the child do not warrant us to believe her to have been more than twelve or thirteen years old, this brings the date of the impregnation to an earlier period than can well be believed. From all theſe circumſtances we might be led to ſuppoſe, that the formation of the hair and teeth was not in conſequence of any connection with a male, but aroſe from ſome action of the ovarium itſelf, in which the uterus did not participate. The exiſtence of the hymen, eſpecially in ſo young a girl, becomes a collateral confirmation of the ſame opinion, although much is not to be reſted on it, when taken ſingly.

It will, perhaps, have ſome influence in removing the prejudices againſt this opinion, to make the following remarks. Hair is occaſionally formed in parts of the human body, which are abſolutely unconnected with generation. Encyſted tumours are ſometimes found containing hair. Mr. Hunter has a preparation of this ſort in his collection, which he cut out from under the ſkin of the eyebrow. This tumour was perfectly complete, and unconnected with the ſkin, except by the common intervention of cellular membrane, ſo as to have no communication whatever with the hair of the eyebrow. In this inſtance there was certainly a ſpecies of generation taking place in the encyſted tumour itſelf, forming hairs as completely and fully as in the common progreſs of the formation of a child. Such encyſted tumours have been found in other parts of the human body, and ſtill more frequently in quadrupeds. Mr. Hunter has in his collection many ſpecimens of encyſted tumours from cows and ſheep containing hair and wool. Theſe were perfectly complete, ſo as to have poſſeſſed a power of production within themſelves, and were many of them found deeply ſeated at a conſiderable diſtance from the ſkin, which is the common parent of hair. In theſe tumours there is often the appearance of layers of cuticle, which is probably a preparatory ſtep to the formation of hair. All this ſhews moſt clearly, that hair may be formed without any ſpecies of generation as it is commonly underſtood.

But hair is in itſelf as diſtinct a conſequence of generation as teeth, and as much a peculiar ſubſtance. If then the one be formed, there appears to be no reaſon why the other ſhould not alſo be formed. The action producing the one is not better underſtood than that producing the other; nor does it appear to be really in itſelf leſs connected with that ſpecies of generation ariſing from the approach of a male, ſo that teeth may probably be formed by a peculiar action taking place in the ovarium itſelf, as well as the hair.

It will tend to add further weight to this opinion, to conſider that many of the adult teeth are formed in a child after birth; and therefore their formation depends on an action taking place in the jaws at a particular period, and not on original growth. The ſame circumſtance ſtrikes more ſtrongly in the occaſional formation of teeth at an advanced time of life. Both of theſe proceſſes take place after the animal has been formed, in conſequence of a certain action being excited in a particular part of the body, and therefore there is leſs difficulty in believing that the ſame ſort of proceſs may go on in another part of the body not commonly employed in it. It ſeems reaſonable alſo to ſuppoſe, that the ovaria ſhould have a greater aptitude of taking on a proceſs ſomewhat ſimilar to generation than the other indifferent parts of the body, as they conſtitute a part of the organs which are ſo materially concerned in the real proceſs itſelf *. Theſe circumſtances, when taken collectively, would

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* As the formation of teeth and hair involved in a fatty maſs may be ſaid to be peculiar to the ovaria, and as there are ſtrong reaſons for believing, that this formation

would ſeem to render it very probable, that the formation of hair and teeth in the ovarium does not neceſſarily depend on a connection between a male and a female (as has been the common opinion), but ariſes from ſome action within the ovarium itſelf, which is imitative of generation.

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formation may take place without an intercouſe between the ſexes, it becomes difficult to account for this peculiarity in them, unleſs by ſuppoſing, that they have a greater aptitude of running into ſuch a proceſs than the other parts of the body.

IX. *Some Account of the Vegetable and Mineral Productions of* Boutan *and* Thibet. *By Mr.* Robert Saunders, *Surgeon at* Boglepoor *in* Bengal; *communicated by Sir* Joſeph Banks, *Bart. P. R. S.*

Read February 26 and March 5, 1789.

ROAD to Buxaduar, May 11 and 12, 1783. The tract of country from Bahar to the foot of the hills contains but few plants that are not common to Bengal. Pine-apples, mango-tree, jack and ſaul timber, are frequently to be met with in the foreſts and jungles. Find many orange-trees towards the foot of the hills, of a very good ſort, and bearing much fruit. Saw a few lime-trees, and found three different ſpecies of the ſenſitive plant. One ſpecies is uſed medicinally by the natives of Bengal in fevers; it is a powerful aſtringent and bitter; another is the ſpecies from which Terra Japonica is made, a medicine the hiſtory of which we are but lately made acquainted with. The third ſpecies is well known as the ſenſitive plant, and common in Bengal.

The country, from Bahar to the foot of the mountains, to which we approach without any aſcent, is rendered one of the moſt unhealthy parts of India, from a variety of cauſes.

The whole, a perfect flat, is at all times wet and ſwampy, with a luxuriant growth of reeds, long graſs, and underwood, in the midſt of ſtagnated water, numerous frogs and inſects. The exhalations from ſuch a ſurface of vegetable matter and ſwamps, increaſed by an additional degree of heat from the reflection of the hills, affect the air to a conſiderable extent, and render it highly injurious to ſtrangers and European conſtitutions.

The thermometer at the foot of the hill, mid-day 86°, fell to 78° at two o'clock, the time we reached Buxaduar, and that hour of the day when it is generally higheſt.

The ſoil and appearance of the ground in aſcending the hill are materially changed. See many looſe ſparry ſtones and rock containing iron. Two ſprings, conducted from a diſtant height by ſpouts, are very pure and good water, without any mineral impregnation. The mountains in view covered with foreſts of trees, rendered uſeleſs from their inacceſſibility. Thoſe peculiar to the country are known to the natives by the names Boumbſhi, Toumbſhi and Sindeſhi, beſides ſaul timber, bamboo, and plantains.

Buxaduar, May 12 to 21. Many of the plants peculiar to Bengal require nurſing at Buxaduar. There is one very good banian tree. In the jungles, met with the ginger, and a very good ſort of yam; ſaw ſome pomegranate-trees in good preſervation; ſhallots in great perfection; a ſpecies of the Lychnis, Arum, and Aſclepias, natives of more northern ſituations, and of little uſe; a bad ſort of raſberry, and a ſpecies of the Glorioſa. The plantains in uſe below do not thrive here. In the jungles they have a plantain-tree producing a very broad leaf, with which they cover their huts; but the fruit is not eaten. See many weeds and long graſs more common to Bengal than any other parts of Boutan.

From the 15th to the 22d, the rains were almoſt inceſſant at Buxaduar. Our People became unhealthy, and were attacked with fevers, which, if neglected in the beginning, proved obſtinate quartans. This was the caſe with ſeveral of the natives whom I had an opportunity of ſeeing. They ſcarcely, however, admit that Buxaduar is unhealthy at any ſeaſon of the year. After allowing for their prejudice, and the poſſibility of the natives ſuffering but little from the bad ſeaſons, I cannot help thinking, that Buxaduar muſt be unhealthy, at leaſt to ſtrangers, from the month of May till towards the end of September. It lies high, but is overtopped by the ſurrounding mountains, covered with foreſts of trees and underwood. In all climates, where the influence of the ſun is great, this is a never-failing cauſe of bad air. The exhalation that takes place from ſo great a ſurface in the daytime falls after ſunſet in the form of dew, rendering the air raw, damp, and chilly, even in the moſt ſultry climates.

The thermometer at Buxaduar was never, at two o'clock in the afternoon, above 82°, or below 73°.

In the neighbourhood of Buxaduar there are ſeveral excellent ſprings of water, ſome of them with leſs impregnation of any ſort than I ever met with; the niceſt teſt ſcarcely produced the ſeparation of a ſenſible quantity of earthy matter. Such waters are generally to be diſtinguiſhed by the taſte, which is inſipid and unpleaſant. When theſe ſprings could be traced to their ſource, they ſunk the thermometer eight or ten degrees below the temperature of the atmoſphere.

Road to Muriſhong, May 22 and 23. In aſcending the hill from Buxaduar there is to be ſeen much of an imperfect quartz, of various forms and colour, having in ſome places the appearance of marble; but from chemical experiments, it was found to poſſeſs very different properties. This ſort of quartz, when of a pure white, and free from any metallic colouring matter, is uſed as an ingredient in porcelain. I have not ſeen any that promiſes to anſwer that purpoſe better than what is to be met with in the mountains near

Buxaduar. It is known to mineralliſts in that ſtate by the name of quartz gritſtone. The rock which forms the baſis of theſe mountains dips in almoſt every direction, and is covered with a rich and fertile ſoil, but in no place level enough to be cultivated. Many European plants are met with on the road to Muriſhong; many different ſorts of moſſes, fern, wild thyme, peaches, willow, chickweed, and graſſes common to the more ſouthern parts of Europe, nettles, thiſtles, dock, ſtrawberry, raſberry, and many deſtructive creepers, ſome peculiar to Europe.

Muriſhong is the firſt pleaſant and healthy ſpot to be met with on this ſide of Boutan. It lies high, and much of the ground about it is cleared and cultivated; the ſoil, rich and fertile, produces good crops. The only plant now under culture is a ſpecies of the Polygonum of LINNÆUS, producing a triangular ſeed, nearly the ſize of barley, and the common food of the inhabitants. It was now the beginning of their harveſt; and the ground yields them, as in other parts of Boutan, a ſecond crop of rice. Here are to be found in the Jungles two ſpecies of the Laurus of LINNÆUS; one known by the name of the baſtard cinnamon. The bark of the root of this plant, when dried, has very much the taſte and flavour of cinnamon; it is uſed medicinally by the natives. The Chenopodium, producing the ſemen ſantonicum, or wormſeed, a medicine formerly in great character, and uſed in thoſe diſeaſes from which it is named, is common here.

Found in the neighbourhood of this place all the European plants we had met with on the road. The aſcent from Buxaduar to Muriſhong is upon the whole great, with a ſenſible change in the ſtate of the air.

Road to Chooka, May 25. On the road to Chooka find all the Muriſhong plants, cinnamon-tree, willow, and one or two firs; ſtrawberries every where, and very good, and a few bilberry plants.

Much ſparry flint, and a ſort of granite with which the road is paved. There is a great deal of talc in the ſtones and ſoil, but in too ſmall pieces to be uſeful. Frequent beds of clay and pure ſand. Find two mineral wells, ſlightly impregnated with iron, with much appearance of that metal in this part of the country; and they are not unacquainted with the method of extracting it from the ſtones, but ſtill deſpiſe its uſe in building. Towards Chooka there are many well cultivated fields of wheat and barley.

Road to Punukha, May 26. From Chooka the country opens, and preſents to view many well cultivated fields and diſtant villages; a rapid change in climate, the vegetable productions, and general appearance of the country. Towards Punukha, pines and firs are the only trees to be met with; but they do not yet ſeem in their proper climate, being dwarfiſh and ill-ſhaped; peaches, raſberries, and ſtrawberries, thriving every where; ſcarce a plant to be ſeen that is not of European growth. In addition to the many I have already mentioned, ſaw two ſpecies of the Cratægus, one not yet deſcribed. Saw two aſh-trees in a very thriving ſtate, the ſtar-thiſtle, and many other weeds, in general natives of the Alps and Switzerland.

Much of the rock to-day is, I find on examination, pure limeſtone; a valuable acquiſition if they did not either deſpiſe its uſe, or were unacquainted with its properties. It was moſt advantageouſly ſituated for being worked, and the pureſt perhaps to be met with. There is likewiſe abundance of fire-wood in this part of the country. In building they would derive great benefit from the uſe of it. Their houſes are lofty, the timbers ſubſtantial, and nothing wanting to make them durable, but their being acquainted with the uſe of lime. As a manure it might probably be uſed to great advantage. Many fields of barley in this part of the country; now the beginning of their harveſt. The thermometer here fell, at four o'clock in the afternoon, to 60°: cold and chilly.

Road to Chepta, May 27. On the road to Chepta, the rock in general dips to the northward and eaſtward, in about an angle of ſixty degrees. Much of limeſtone, and ſome veins of quartz, and looſe pieces of ſparry flint ſtriking fire with ſteel.

Several ſprings, and one ſlightly impregnated with iron.

In addition to the plants of yeſterday, find the Coriandrum teſticulatum, Inula montana, and Rhododendum magnum.

At Chepta met with a few turneps, one maple-tree, wormwood, gooſe-graſs (Galium aparine), and many other European weeds; the firſt walnut-tree we had yet ſeen.

Chepta lies high, and not above ſix miles from the mountain of Lomyla, now covered with ſnow. The wind from that quarter, S.E. made it cold and chilly, and ſunk the thermometer at mid-day to 57°. Here are ſome fields of wheat and barley not yet ripe.

Road to Pagha, May 29. Soon after leaving Chepta find a mineral well, which, on a chemical examination, gave marks of a ſtrong impregnation from iron. I traced it to its ſource, where the thermometer, on being immerſed, fell from 68° to 56°.

A little before we reach Pagha, met with ſome limeſtone, and a bed of chalk, which, near the ſurface, contained a great proportion of ſand, but ſome feet under was much purer.

The foreſts of firs of an inferior growth, ſeveral aſh-trees, dog-roſe, and bramble.

Road to Taſſeſudon, May 30 and 31, June 1. The road from here to Taſſeſudon preſents us with little that we have not met with; fewer ſtrawberries, and no raſberries; ſome very good orchards of peaches, apricots, apples, and pears. The fruit formed, and will be ripe in Auguſt and September. Met with two ſorts of cranberry, one very good. Saw, the Fragaria ſterilis, and a few poppies. At Wanakha found a few turneps, ſhallots, cucumbers, and gourds. Near to Taſſeſudon the road is lined with many different ſpecies of the roſe, and a few jeſſamine plants. The ſoil is light, and the hills in many places barren, rocky, and with very little verdure. The rock in general laminated and rotten, with many ſmall particles of talc in every part of the country incorporated with the ſtones and ſoil. Some limeſtone, and appearance of good chalk. Several good and pure ſprings of water.

Taſſeſudon and its neighbourhood abound with all the plants we have already mentioned. The hills are chiefly wood, with firs and aſpen. I have not yet been able to find an oak-tree, and the aſh is very ſeldom to be met with. The elder, holly, bramble, and dog-roſe, are common. Found the birch-tree, cypreſs, yew, and delphinium. Many different ſpecies of the vaccinium, of which the bilberry is one, and the cranberry another. Towards the top of the adjacent mountains met with two plants of the Arbutus uva urſi, which is a native of the Alps, the moſt mountainous parts of Scotland, and Canada.

I have likewiſe ſeen a ſpecies of the rhubarb plant (Rheum undulatum) brought from a diſtance, and only to be met with near the ſummits of hills covered with ſnow, and where the ſoil is rocky. The true rhubarb (Rheum palmatum) is likewiſe the native of a cold climate; and though China ſupplies us with much of this drug, it is known to be the growth of its more northern provinces, Tartary, and part of the Ruſſian dominions. The great difficulty is in drying the root. People verſant in that buſineſs ſay, that one hundred pounds of the freſh root ſhould not weigh above ſix pounds and a half, if properly dried; and it certainly has been reduced to that. I have ſeen eighty pounds of freſh root produced from one plant; but, after drying it with much care and attention, the weight of the dried root could not be made leſs than twelve pounds. It was ſuſpended in an oven, with an equal and moderate degree of heat. Little more than the ſame quantity of this powder produced a ſimilar effect with the beſt foreign rhubarb.

The other plants common here are the ſervice-tree, bleſſed thiſtle, mock orange, Spiræa filipendula, Arum, Echites, Punica, Ferula communis, Erica, and Viola. Of the roſe-buſh I have met with the five following ſpecies; Roſa alpina, centifolia, canina, Indica, ſpinoſiſſima.

The culture of pot-herbs is every where neglected; turneps, a few onions and ſhalots, were the beſt we could procure. Mr. BOGLE left potatoes, cabbage, and lettuce-plants, all which we found neglected and diſperſed. They had very improperly (from an idea moſt probably of their being natives of Bengal) planted them in a ſituation and climate which approaches very near to that of Bengal at all ſeaſons, as we ſhall find afterwards. Melons, gourds, brinjals, and cucumbers, are occaſionally to be met with. The country is fitted for the production of every fruit and vegetable common without the tropics, and in ſome ſituations will bring to perfection many of the tropical fruits.

There are two plants which I have to regret the not having had as yet an opportunity of ſeeing; one is the tree from the bark of which their paper is made; and the other is employed by them in poiſoning their arrows. This laſt is ſaid to come from a very remote part of the country. They deſcribe it as growing to the height of three or four feet, with a hollow ſtalk. The juice is inſpiſſated, and laid as a paſte on their arrows. Fortunately for them, it has not all the bad effects they dread from it. I had an opportunity of ſeeing ſeveral who were wounded with theſe arrows, and they all did well, though under the greateſt apprehenſion. The cleaning and enlarging ſome of the wounds was the moſt that I found neceſſary to be done. The paſte is pungent and acrid, will increaſe inflammation, and may make a bad or neglected wound mortal; but it certainly does not poſſeſs any ſpecific quality as a poiſon.

The fir, ſo common in this country, is perhaps the only tree they could convert to a uſeful and profitable purpoſe. What I have ſeen would not, from their ſituation, be employed as timber. The largeſt I have yet met with were near Wandepore; they meaſured from eight to ten feet in circumference, were tall and ſtraight. Such near the Burrampooter, or any navigable river, might certainly be tranſported to an advantageous market. I am convinced that any quantity of tar, pitch, turpentine, and reſin, might be made in this country, much to the emolument of the natives. Firs, which from their ſize and ſituation are unfit for timber, would anſwer the purpoſe equally well. The proceſs for procuring tar and turpentine is ſimple, and does not require the conſtruction of expenſive works. This great object has been ſo little attended to, that they are ſupplied from Bengal with what they want of theſe articles.

The country about Taſſeſudon contains great variety of ſoil, and much rock of many different forms, but ſtill an unpromiſing field for a mineraliſt. I have not found in Boutan a foſſil that had the leaſt appearance of containing any other metal than iron, and a ſmall portion of copper. From information, and the reports of travellers, I believe it is otherwiſe to the northward. The banks of the Ticuſhu, admitting of cultivation for ſeveral miles above and below Taſſeſudon, yield them two crops in the year. The firſt of wheat and barley is cut down in June; and the rice, planted immediately after, enjoys the benefit of the rains. This country is not without its hot wells, as well as many numerous ſprings, ſome of which I have taken notice of. One hot well, near Wandepore, is ſo cloſe to the banks of the river as to be overflowed in the rains, and we found it impoſſible to get to it: the heat of this well is great; but I could not learn that the ground about it was much different from the general aſpect of the country. Another, ſeveral days journey from hence, is on the brow of a hill perpetually covered with ſnow. This hot well is held in great eſtimation by the people of the country, and reſorted to by valetudinarians of every deſcription. I gained but little ſatisfactory information reſpecting the degree of heat, or appearance of the ground about it, that could lead me to form a juſt opinion of either.

Taſſeſudon to Paraghon, Sept. 8 and 9. Left Taſſeſudon, and arrived next day at Paraghon. Much good rich ſoil, with more paſture, where the ground is not cultivated, than we had yet met with. Many fields of turneps in great perfection; a plant they ſeem better acquainted with the cultivation of than any other. Find on the road many large and well-thriving birch, willows, pines, and firs, ſome walnut-trees, the

Arbutus uva urſi, abundance of ſtrawberry, elderberry, bilberry, Chryſanthemum or greater daiſy, and many European graſſes. See the Datura ferox or thorn-apple, a plant common in China and ſome parts of Thibet, where it is uſed medicinally. They find it a powerful narcotic, and give the ſeeds where they wiſh that effect to be produced. It has been uſed as a medicine in Europe, and is known to poſſeſs theſe qualities in a high degree. See holly, dog-roſe, and aſpen. The preſent crop near Paraghon, on the banks of the Pachu, is rice, but not ſo far advanced as at Taſſeſudon; the ſame may be ſaid of their fruits. They ſay it is colder here at all ſeaſons than at Taſſeſudon, which is certainly below the level of this place.

Towards the ſummit of the mountain we croſſed, found ſome rock of a curious appearance, forming in front ſix or ſeven angular ſemi-pillars, of a great circumference, and ſome hundred feet high. This natural curioſity was detached in part from the mountain, and projected over a conſiderable fall of water, which added much to the beautiful and pictureſque appearance of the whole. Numerous ſprings, ſome degrees colder than the ſurrounding atmoſphere, guſhing from the rock on the moſt elevated part of the mountain, furniſh a very ample and ſeaſonable ſupply of excellent water to the traveller. The rock, in many places laminated, might be formed into very tolerable ſlate. Near to Paraghon iron ſtones are found, and one ſpring highly impregnated with this mineral.

Road to Dukaigun, Sept. 11. Our road to Dukaigun, nearly due north, is a continued aſcent for eight miles, along the banks of the Pachu, falling over numerous rocks, precipices, and huge ſtones. Here we begin to experience a very conſiderable change in the temperature of the atmoſphere; the ſurrounding hills were covered with ſnow in the morning, which had fallen the preceding night, but diſappeared ſoon after ſunriſe. The thermometer fell to 54° in the afternoon, and did not riſe above 62° at noon.

The face of the mountains, in ſome places bare, with projecting rock of many different forms; quartz, flint, and a bad ſort of freeſtone, common. Many very good ſprings, ſlightly impregnated with a ſelenitic earth.

The ſoil is rich, and near to the river in great cultivation. Many horſes, the ſtaple article of their trade, are bred in this part of the country. Found walnut-trees, peaches, apples, and pears.

Road to Sanha, Sept. 12. The road ſtill aſcending to Sanha, and near to the river for ten miles.

The thermometer falling ſome degrees, we found it cold and chilly. The bed of the river is full of large ſtones, probably waſhed down from the mountains by the rapidity of its ſtream; they are chiefly quartz and granite. Here is excellent paſture for numerous herds of goats.

Road to Chichakumboo. From Sanha the aſcent is much greater, and, after keeping for ten miles along the banks of the Pachu, ſtill a conſiderable ſtream, we reach its ſource (from three diſtinct rivulets, all in view, ramified and ſupplied by numerous ſprings), and ſoon after arrive at the moſt elevated part of our road.

Here we quit the boundary of Boutan, and enter the territory of Thibet, where nature has drawn the line ſtill more ſtrongly, and affords, perhaps, the moſt extraordinary contraſt that takes place on the face of the earth. From this eminence are to be ſeen the mountains of Boutan, covered with trees, ſhrubs, and verdure to their tops, and on the ſouth ſide of this mountain to within a few feet of the ground on which we tread. On the north ſide the eye takes in an extenſive range of hills and plains, but not a tree, ſhrub, or ſcarce a tuft of graſs to be ſeen. Thus, in the courſe of leſs than a mile, we bid adieu to a moſt fertile ſoil, covered with perpetual verdure, and enter a country where the ſoil and climate ſeem inimical to the production of every vegetable. The change in the temperature of the air is equally obvious and rapid. The thermometer in the forenoon 34°, with froſt and ſnow in the night-time. Our preſent obſervations on the cauſe of this change confirmed us in a former opinion, and inconteſtably prove, that we are to look for that difference of climate from the ſituation of the ground as more or leſs above the general level of the earth. In attending to this cauſe of heat or cold, we muſt not allow ourſelves to be deceived by a compariſon with that which is immediately in view. We ought to take in a greater range of country, and where the road is near the banks of a river, we cannot well err in forming a judgement of the inclination of the ground. Punukha and Wandepore, both to the northward of Taſſeſudon, are quite in a Bengal climate. The thermometer at the firſt of theſe places, in the months of July and January, was within two degrees of what it had been at Rungpore for the ſame periods. They ſeem in more expoſed ſituations than Taſſeſudon ; and, were we to draw a compariſon of their heights from the ſurrounding ground, I ſhould ſay they were above its level. The road, however, proves the reverſe. From Punukha to

Taſſiſudon we had a continued and ſteep aſcent for ſix hours and a half, with a very inconſiderable deſcent on the Taſſiſudon ſide. From the ſouth ſide of the mountain dividing Boutan from Thibet, the ſprings and rivulets are tumbling down in caſcades and torrents, and have been traced by us near to the foot of the hills, where they empty themſelves to the eaſtward of Buxaduar. On the north ſide they glide ſmoothly along, and by paſſing to the northward as far as Tiſhoolumboo, prove a deſcent on that ſide, which the eye could not detect. This part of the country, being the moſt elevated, is at all times the coldeſt; and the ſnowy mountains, from their heights and bearings, notwithſtanding the diſtance, are certainly thoſe ſeen from Purnea.

The ſoil on the Thibet ſide of the mountain is ſandy, with much gravel and many looſe ſtones. On the road found the Aconitum pyreneum, and two ſpecies of the Saxifraga.

See a large flock of chowry tailed cattle; their extenſive range of paſture ſeems to make amends for its poverty.

From Faro to Duina, Sept. 15. From Faro to Duina paſs over an extenſive plain, bounded by many ſmall hills, oddly arranged; ſome of them detached and ſingle, and all ſeem compoſed of ſand collected in that form, having the plain for their general baſe.

At Duina found a few plots of barley, which they are now cutting down, though green, as deſpairing of its ripening. The thermometer, at ſix o'clock in the morning, below the freezing point, and the ground partially covered with ſnow.

Road to Chalu, Sept. 16. Continue on the plain; find three ſprings forcing their way through the ground with violence, and giving riſe to a lake many miles in extent, ſtored with millions of water-fowl and excellent fiſh. Of the firſt ſaw the cyras, ſolan geeſe, many kinds of ducks, pintados, cranes, and gulls of different ſorts. [1] The ſprings of this lake are in great reputation for the cure of moſt diſeaſes. I examined the water, and found it contain a portion of alum with the ſelenitic earth. On the banks of the lake I found a cryſtallization, which proves to be an alkaline ſalt; it is uſed by the natives for waſhing, and anſwers the purpoſe as well as pot-aſh. The paſture which is impregnated with this ſalt is greedily ſought after by ſheep and goats, and proves excellent food for them. The hills are chiefly compoſed of ſand incruſted by the inclemency of the weather and violent winds, ſeeming at firſt view compoſed of freeſtone.

Road to Simadar, Sept. 17. Paſs a lake ſtill more conſiderable than the former, with which it communicates by a narrow ſtream, about three miles long. There never was a more barren or unpromiſing ſoil; little turf, graſs, or vegetation of any ſort, except near the lake. See a few huts, moſtly in ruins and deſerted. The only grain in this part of the country is barley, which they are cutting down every where green.

Paſs two ſprings, one of them ſlightly impregnated with alum. They form the principal ſource of a river, which empties itſelf in the Burrampooter near Tiſſoolumboo.

The wind from the eaſtward of ſouth is now the coldeſt and moſt piercing; paſſing over the ſnowy mountains and dry ſandy deſart before deſcribed, it comes diveſted of all vapour or moiſture, and produces the ſame effect as the hot dry winds in more ſoutherly ſituations. Mahogany boxes and furniture, that had withſtood the Bengal climate for years, were warped with conſiderable fiſſures, and rendered uſeleſs. The natives ſay, a direct expoſure to theſe winds occaſions the loſs of their fore-teeth; and our faithful guide aſcribed that defect in himſelf to this cauſe. We eſcaped with loſs of the ſkin from the greateſt part of our faces.

Road to Seluh, Sept. 18. Near our road to-day found a hot well, much frequented by people with venereal complaints, rheumatiſm, and all cutaneous diſeaſes. They do not drink the water, but uſe it as a bath. The thermometer, when immerſed in the water, roſe from 40° to 88°. It has a ſtrong ſulphureous ſmell, and contains a portion of hepar ſulphuris. Expoſure to air deprives it, as moſt other mineral wells, of much of its property.

Road to Takui, Sept. 19. Paſs ſome fields of barley and peaſe, and get into a milder climate. Find to-day a great variety of ſtone and rock, ſome containing copper, and others, a very pure rock cryſtal, regularly cryſtallized, with ſix unequal ſides. The rock cryſtal is of different ſizes and degrees of purity, but of one form. Find ſome flint and granite, ſeveral ſprings of water impregnated with iron, and nearly of the ſame temperature with the atmoſphere. See a few ill-thriving willows planted near the habitations, and which are the only trees to be met with.

Road to Tiſſoolumboo, Sept. 20, 21, and 22. The remaining part of our journey is over a more fertile ſoil, enjoying a milder climate. Some very good fields of wheat, barley, and peaſe; many pleaſant villages and diſtant houſes, leſs ſand and more rock, part ſlaty, and much of it a very good ſort of flint. The ſoil in the valley a light-coloured clay and ſand. They are every where employed in cutting down their crop. What a happy climate! The ſky ſerene and clear, without a cloud; and ſo confident are they of the continuance of this weather, that their crop is thrown together in a convenient part of the field, without any cover, to remain till they can find time to threſh it out,

Before we reached Tiſſoolumboo found ſome elms and aſh-trees.

The hills in Thibet have, from their general appearance, ſtrong marks of containing thoſe foſſils that are inimical to vegation; ſuch are moſt of the ores of metal and pyritical matter.

The country, properly explored, promiſes better than any I have ſeen to gratify the curioſity of a philoſopher, and reward the labours of a mineraliſt. Accident, more than a ſpirit of enterpriſe and enquiry, has already diſcovered the preſence of many valuable ores and minerals in Thibet. The firſt in this liſt is deſervedly gold. They find it in large quantities, and frequently very pure. In the form of gold-duſt it is found in the beds of rivers, and at their ſeveral bendings, generally attached to ſmall pieces of ſtone, with every appearance of its having been part of a larger maſs. They find it ſometimes in large maſſes, lumps, and irregular veins; the adhering ſtone is generally flint or quartz, and I have ſometimes ſeen a half-formed, impure ſort of precious ſtone in the maſs. By a common proceſs for the purification of gold, I extracted 12 *per cent.* of refuſe from ſome gold-duſt, and on examination found it to be ſand and filings of iron, which laſt was not likely to have been with it in its native ſtate, but probably employed for the purpoſe of adulteration. Two days journey from Tiſſoolumboo there is a lead mine. The ore is much the ſame as that found in Derbyſhire, mineralized by ſulphur, and the metal obtained by the very ſimple operation of fuſion alone. Moſt lead contains a portion of ſilver, and ſome in the proportion to make it an object to work the lead ore for the ſake of the ſilver. Cinnabar, containing a large proportion of quickſilver, is found in Thibet, and might be advantageouſly employed for the purpoſe of extracting this metal. The proceſs is ſimple, by diſtillation; but to carry it on in the great would require more fuel than the country can well ſupply. I have ſeen ores and looſe ſtones containing copper, and have not a doubt of its being to be found in great abundance in the country. Iron is more frequently to be met with in Boutan than Thibet; and, was it more common, the difficulty of procuring proper fuel for ſmelting the leſs valuable ores, muſt prove an inſuperable objection to the working them. The dung of animals is the only ſubſtitute they have for fire-wood, and with that alone they will never be able to excite a degree of heat ſufficiently intenſe for ſuch purpoſes. Thus ſituated, the moſt valuable diſcovery for them would be that of a coal mine. In ſome parts of China bordering on Thibet, coal is found and uſed as fuel.

Tincal, the nature and production of which we have only hitherto been able to gueſs at, is now well known, and Thibet, from whence we are ſupplied, contains it in inexhauſtible quantities. It is a foſſil brought to market in the ſtate it is dug out of the lake, and afterwards refined into Borax by ourſelves. Rock ſalt is likewiſe found in great abundance in Thibet.

The lake, from whence tincal and rock ſalt are collected, is about fifteen days journey from Tiſſoolumboo, and to the northward of it. It is encompaſſed on all ſides by rocky hills, without any brooks or rivulets near at hand; but its waters are ſupplied by ſprings, which being ſaltiſh to the taſte are not uſed by the natives. The tincal is depoſited or formed in the bed of the lake; and thoſe who go to collect it, dig it up in large maſſes, which they afterwards break into ſmall pieces for the convenience of carriage, expoſing it to the air to dry. Although tincal has been collected from this lake for a great length of time, the quantity is not perceptibly diminiſhed; and as the cavities made by digging it ſoon wear out or fill up, it is an opinion with the people, that the formation of freſh tincal is going on. They have never yet met with it in dry ground or high ſituations, but it is found in the ſhalloweſt depths, and the borders of the lake, which deepening gradually from the edges towards the center contains too much water to admit of their ſearching for the tincal conveniently; but from the deepeſt parts they bring up rock ſalt, which is not to be found in the ſhallows, or near the bank. The waters of the lake riſe and fall very little, being ſupplied by a conſtant and unvarying ſource, neither augmented by the influx of any current, or diminiſhed by any ſtream running from it. The lake, I am aſſured, is at leaſt twenty miles in circumference, and ſtanding in a very bleak ſituation is frozen for a great part of the year. The people employed in collecting theſe ſalts are obliged to deſert from their labour ſo early as October, on account of the ice. Tincal is uſed in Thibet for ſoldering and to promote the fuſion of gold and ſilver. Rock ſalt is univerſally uſed for all domeſtic purpoſes in Thibet, Boutan, and Naphaul.

The thermometer at Tiſſoolumboo during the month of October was, on an average, at eight o'clock in the morning 38°, at noon 46°, and ſix o'clock in the evening 42°. The weather clear, cool, and pleaſant, and the prevailing wind from the ſouthward. During the month of November we had froſts morning and evening, a ſerene clear ſky, not a cloud to be ſeen. The rays of the ſun paſſing through a medium ſo little obſcured had great influence. The thermometer was often below 30° in the morning, and ſeldom above 38° at noon in the ſhade; wind from the ſouthward.

Of the diſeaſes of this country, the firſt that attracts our notice, as we approach the foot of the hills, is a glandular ſwelling in the throat, which is known to prevail in ſimilar ſituations in ſome parts of Europe, and generally aſcribed to an impregnation of the water from ſnow. The diſeaſe being common at the foot of the Alps, and confined to a tract of country near theſe mountains, has firſt given riſe to the idea of its being occaſioned by ſnow water. If a general view of the diſeaſe, and ſituations where it is common, had been the ſubject of enquiry, or awakened the attention of any able practitioner, we ſhould have been long ſince undeceived in this reſpect. On the coaſt of Greenland, the mountainous parts of Wales and Scotland, where melted ſnow muſt be continually paſſing into their rivers and ſtreams, the diſeaſe is not known, though it is common in Derbyſhire, and ſome other parts of England. Rungpore is about one hundred miles from the foot of the hills, and much farther from the ſnow, yet the diſeaſe is as frequent there as in Boutan. In Thibet, where ſnow is never out of view, and the principal ſource of all their rivers and ſtreams, the diſeaſe is not to be met with; but what puts the matter paſt a doubt, is the frequency of the diſeaſe on the coaſt of Sumatra, where ſnow is never to be found. On finding the vegetable productions of Boutan the ſame as thoſe of the Alps in almoſt every inſtance, it occurred to me, that the diſeaſe might ariſe from an impregnation of the water by theſe plants, or the ſoil probably poſſeſſing ſimilar qualities, the ſpontaneous productions of both countries, with very few exceptions, being ſo nearly alike. It however appears more probable, that the diſeaſe is endemial, proceeding from a peculiarity in the air of ſituations in the vicinity of mountains with ſuch ſoil and vegetable productions. I am the more inclined to think ſo, that I have univerſally found this diſeaſe moſt prevalent amongſt the lower claſs of people, and thoſe who are moſt expoſed to the unguarded influence of the weather, and various changes that take place in the air of ſuch ſituations. The primary cauſe in the atmoſphere producing this effect is, perhaps, not more inexplicable than what we meet with in the low-lands of Eſſex and fens of Lincolnſhire. An accurate analyſis of the water uſed in common by the natives, where this diſeaſe is more or leſs frequent, and where it is not known in ſimilar expoſures, might throw ſome light on this ſubject.

This very extraordinary diſeaſe has been little attended to, from obvious reaſons; it is unaccompanied with pain, ſeldom fatal, and generally confined to the poorer ſort of people. The tumor is unſightly, and grows to a troubleſome ſize, being often as large as a perſon's head. It is certainly not exaggerating to ſay, that one in ſix of the Rungpore diſtrict, and country of Boutan, has the diſeaſe.

As thoſe who labour moſt, and are the leaſt protected from the changes of weather, are moſt ſubject to the diſeaſe, we univerſally find it in Boutan more common with the women than men. It generally appears in Boutan at the age of thirteen or fourteen, and in Bengal at the age of eleven or twelve; ſo that in both countries the diſeaſe ſhows itſelf about the age of puberty. I do not believe this diſeaſe has ever been removed, though a mercurial courſe ſeemed to check its progreſs, but did not prevent its advance after intermitting the uſe of mercury. An attention to the primary cauſe will firſt lead to a proper method of treating the diſeaſe; a change of ſituation for a ſhort while, at that particular period when it appears, might be the means of preventing it.

The people of this happy climate are not exempt from the venereal diſeaſe, which ſeems to rage with unremitting fury in all climates, and proves the greateſt ſcourge to the human race. It has been long a matter of doubt, whether this diſeaſe has ever been cured by any other ſpecific than mercury and its different preparations. In defence of the opinion of other ſpecifics being in uſe, it has always been urged, that the diſeaſe is frequent in many parts of the world, where it could not be ſuppoſed that they were acquainted with quickſilver, and the proper method of preparing it as a medicine. I muſt own, that I expected to have been able to have added one other ſpecific for this diſeaſe to our liſt in the Materia medica, being informed that the diſeaſe was common, and their method of treating it ſucceſsful; nor could I allow myſelf to think they were acquainted with the method of preparing quickſilver, ſo as to render it a ſafe and efficacious medicine. In this, however, I was miſtaken.

The diſeaſe ſeems in this country to make a more rapid progreſs, and rage with more violence, than in any other. This is to be accounted for from the groſsneſs of their food and little attention to cleanlineſs.

There is one preparation of mercury in common uſe with them, and made after the following manner. A portion of alum, nitre, vermillion, and quickſilver, are placed in the bottom of an earthen pot, with a ſmaller one inverted put over the materials, and well luted to the bottom of the larger pot. Over the ſmall one, and within the large one, the fuel is placed, and the fire continued for about forty minutes. A certain quantity of fuel, carefully weighed out, is what regulates them with reſpect to the degree of heat, as they cannot ſee the materials during the operation. When the veſſel is cool, the ſmall inverted pot is taken off, and the materials collected for uſe. I attended the whole of the proceſs, and examined the materials afterwards. The quickſilver had been acted on by the other ingredients, deprived of its metallic form, and rendered a ſafe and efficacious remedy.

A knowledge of chemiſtry has taught us a more certain method of rendering this valuable medicine active and efficacious; yet we find this preparation anſwering every good purpoſe, and by their guarded manner of exhibiting it perfectly ſafe. This powder is the baſis of their pill, and often uſed in external application. The whole, when intimately mixed, formed a reddiſh powder, and was made into the form of pills by the addition of a plum or date. Two or three pills taken twice a day generally bring on, about the fourth or fifth day, a ſpitting, which is encouraged by continuing the uſe of the pills for a day or two longer. As the ſalivation advances, they put a ſtick acroſs the patient's mouth, in the form of a gag, and make it faſt behind. This, they ſay, is done to promote the ſpitting, and prevent the loſs of their teeth. They keep up the ſalivation for ten or twelve days, during which time the patient is nouriſhed with congee and other liquids. Part of this powder is often uſed externally by diffuſing it in warm water, and waſhing ſores and buboes. They diſperſe buboes frequently by poultices of turnip tops, in which they always put vermillion, and ſometimes muſk. Nitre, as a cooler, is very much uſed internally by them in this diſeaſe, and they ſtrictly enjoin warmth and confinement during the ſlighteſt mercurial courſe. Buboes advanced to ſuppuration are opened by a lancet, with a large inciſion, which they do not allow to cloſe before the hardneſs and tumor are gone. In ſhort, I found very little room for improving their practice in this diſeaſe. I introduced the method of killing quickſilver with honey, gave them an opportunity of ſeeing it done, and had the ſatisfaction of finding it ſucceſsfully uſed by themſelves before we left the country.

This happy climate preſents us with but little variety in their diſeaſes. Coughs, colds, and rheumatiſm, are more frequent here than in Bengal. Fevers generally ariſe here from a temporary cauſe, are eaſily removed, and ſeldom prove fatal. The liver diſeaſe is occaſionally to be met with, and complaints in the bowels are not unfrequent; but the groſſneſs of their food, and uncleanlineſs of their perſons, would in any other climate be the ſource of conſtant diſeaſe and ſickneſs. They are ignorant (as we were, not many years ago) of the proper method of treating diſeaſes of the liver and other viſcera; this is, I believe, the cauſe of the moſt obſtinate and fatal diſeaſe to be met with in the country, I mean the dropſy. As the Rajah had ever been deſirous of my aid and advice, and had directed his doctors to attend to my private inſtructions and practice, I endeavoured to introduce a more judicious method of treating thoſe diſeaſes by mercurial preparations. I had an opportunity of proving the advantage of this plan to their conviction in ſeveral inſtances, and of ſeeing them initiated in the practice.

The Rajah favoured me with above ſeventy ſpecimens of the medicines in uſe with them. They have many ſorts of ſtones and petrifactions ſaponaceous to the touch, which are employed as an external application in ſwellings and pains of the joints. They often remove ſuch complaints, and violent head-achs, by fumigating the part affected with aromatic plants and flowers. They do not ſeek for any other means of information reſpecting the ſtate of a patient than that of feeling the pulſe; and they confidently ſay, that the ſeat of pain and diſeaſe is eaſily to be diſcovered, not ſo much from the frequency of the pulſe as its vibratory motion. They feel the pulſe at the wriſt with their three fore-fingers, firſt of the right, and then of the left hand; after preſſing more or leſs on the artery, and occaſionally removing one or two of the fingers, they determine what the diſeaſe is. They do not eat any thing the day on which they take phyſic, but endeavour to make up the loſs afterwards by eating more freely than before, and uſing ſuch medicines as they think will occaſion coſtiveneſs.

The many ſimples in uſe with them are from the vegetable kingdom, collected chiefly in Boutan. They are in general inoffenſive and very mild in their operation. Carminatives and aromatics are given in coughs, colds, and affections of the breaſt. The centaury, coriander, carraway, and cinnamon, are of this ſort. This laſt is with them the bark of the root of that ſpecies of Laurus formerly mentioned as a native of this country. The bark from the root is in this plant the only part which partakes of the cinnamon taſte; and I doubt very much if it could be diſtinguiſhed by the beſt judges from what we call the true cinnamon. The bark, leaves, berries, and ſtalks of many ſhrubs and trees, are in uſe with them, all in decoction. Some have much of the aſtringent bitter taſte of our moſt valuable medicines, and are generally employed here with the ſame view, to ſtrengthen the powers of digeſtion, and mend the general habit. Their principal purgative medicines are brought by the Chineſe to Laſſa. They had not any medicine that operated as a vomit, till I gave the Rajah ſome ipecacuanha, who made the firſt experiment with it on himſelf.

In bleeding they have a great opinion of drawing the blood from a particular part. For head-achs they bleed in the neck; for pains in the arm and ſhoulder, in the cephalic vein; and of the breaſt or ſide, in the median; and if in the belly, they bleed in the baſilic vein. They think pains of the lower extremity are beſt removed by bleeding in the ancle. They have a great prejudice againſt bleeding in cold weather; nor is any urgency or violent ſymptom thought at that time a ſufficient reaſon for doing it.

They have their lucky and unlucky days for operating or taking any medicine; but I have known them get the better of this prejudice, and be prevailed on.

Cupping is much practiſed by them; a horn, about the ſize of a cupping glaſs, is applied to the part, and by a ſmall aperture at the other end they extract the air with their mouth. The part is afterwards ſcarified with a lancet. This is often done on the back; and in pain and ſwelling of the knee it is held as a ſovereign remedy. I have often admired their dexterity in operating with bad inſtruments. Mr. HAMILTON gave them ſome lancets, and they have ſince endeavoured, with ſome ſucceſs, to make them of that form. They were very thankful for the few I could ſpare them. In fevers they uſe the Kuthullega nut, well known in Bengal as an efficacious medicine. They endeavour to cure the dropſy by external applications, and giving a compounded medicine made up of above thirty different ingredients: they ſeldom or never ſucceed in effecting a cure of this diſeaſe.

I explained to the Rajah the operation of tapping, and ſhewed him the inſtrument with which it was done. He very earneſtly expreſſed a deſire that I ſhould perform the operation, and wiſhed much for a proper ſubject; ſuch a one did not occur while I remained, and perhaps it was as well both for the Rajah's patients and my own credit; for after having ſeen it once done, he would not have heſitated about a repetition of the operation. Graveliſh complaints and the ſtone in the bladder are, I believe, diſeaſes unknown here.

The ſmall-pox, when it appears among them, is a diſeaſe that ſtrikes them with too much terror and conſternation to admit of their treating it properly. Their attention is not employed in ſaving the lives of the infected, but in preſerving themſelves from the diſeaſe. All communication with the infected is ſtrictly forbidden, even at the riſk of their being ſtarved, and the houſe or village is afterwards eraſed. A promiſcuous and free intercourſe with their neighbours not being allowed, the diſeaſe is very ſeldom to be met with, and its progreſs always checked by the vigilance and terror of the natives. Few in the country have had the diſeaſe. Inoculation, if ever introduced, muſt be very general to prevent the devaſtation that would be made by the infection in the natural way; and where there could not be any choice in the ſubject fit to receive the diſeaſe, many muſt fall a ſacrifice to it. The preſent Rajah of Thibet was inoculated, with ſome of his followers, when in China with the late Tiſhoo Lama.

The hot bath is uſed in many diſorders, particularly in complaints of the bowels and cutaneous eruptions. The hot wells of Thibet are reſorted to by thouſands. In Boutan they ſubſtitute water warmed by hot ſtones thrown into it.

In Thibet the natives are more ſubject to ſore eyes and blindneſs than in Boutan. The high winds, ſandy ſoil, and glare from the reflection of the ſun, both from the ſnow and ſand, account for this.

I have dwelt long on this ſubject, becauſe I think the knowledge and obſervations of theſe people on the diſeaſes of their country, with their medical practice, keep pace with a refinement and ſtate of civilization, which ſtruck me with wonder, and no doubt will give riſe to much curious ſpeculation, when known to be the manners of a people holding ſo little intercourſe with what we term civilized nations.

Dec. 1. Left Tiſhoolumbo, and found the cold increaſe every day as we advanced to the ſouthward, moſt of the running waters frozen, and the pools covered with ice ſtrong enough to carry. Our thermometer having only the ſcale as low as 16°, we could not preciſely determine the degree of cold, the quickſilver being under that every morning. The froſt is certainly never ſo intenſe in Great-Britain. On our return to the lakes the 14th, we found them deſerted by the water fowl, and were informed that they had been one ſolid piece of ice ſince the 10th of November. Here we reſumed our amuſement of ſkating, to the great aſtoniſhment of the natives and Bengal ſervants.

On the 17th we re-entered Boutan, and in ſix days more arrived at Punukha by Paraghon. No ſnow or froſt to be met with in Boutan, except towards the tops of their higheſt mountains; the thermometer riſing to 36° in the morning, and 48° at noon.

Took leave of the Debe Rajah, and on the 12th arrived at Buxaduar.

Calcutta, Feb. 17, 1784.

AS Lac is the produce of, and a ſtaple article of commerce in Aſſam, a country bordering on and much connected with Thibet, ſome account of it may not be an improper ſupplement to the above remarks.

Lac is, ſtrictly ſpeaking, neither a gummy nor reſinous ſubſtance; though it has ſome properties in common to both. Gums are ſoluble in water, and reſins in ſpirits; lac admits of a very difficult union with either, without the mediation of ſome other agent.

Lac is known in Europe by the different appellations of ſtick lac, ſeed lac, and ſhell lac. The firſt is the lac in pretty conſiderable lumps, with much of the woody parts of the branches on which it is formed adhering to it. Seed lac is only the ſtick lac broke into ſmall pieces, garbled, and appearing in a granulated form. Shell lac is the purified lac, by a very ſimple proceſs to be mentioned afterward.

Many vague and unauthenticated reports concerning lac have reached the public; and though amongſt the multiplicity of accounts the true hiſtory of this ſubſtance has been nearly hit on, little credit is given in Europe to any deſcription of it hitherto publiſhed. My obſervations, as far as they go, are the reſult of what I have ſeen, from the lac on the tree, the progreſs of the inſect now in my cuſtody, and the information of a gentleman reſiding at Goalpara on the borders of Aſſam, who is perfectly verſant in the method of breeding the inſect, inviting it to the tree, collecting the lac from the branches, and forming it into ſhell lac, in which ſtate much of it is received from Aſſam, and exported to Europe for various great and uſeful purpoſes. The tree on which this fly moſt commonly generates is known in Bengal by the name of the *Biher* tree, and is a ſpecies of the Rhamnus. The fly is nouriſhed by the tree, and there depoſits its eggs, which nature has provided it with the means of defending from external injury by a collection of this lac, evidently ſerving the twofold purpoſe of a nidus and covering to the ovum and inſect in its firſt ſtage, and food for the maggot in its more advanced ſtate. The lac is formed into complete cells, finiſhed with as much regularity and art as a honey-comb, but differently arranged. The flies are invited to depoſit their eggs on the branches of the tree, by beſmearing them with ſome of the freſh lac ſteeped in water, which attracts the fly, and gives a better and larger crop.

The lac is collected twice a year, in the months of February and Auguſt.

I have examined the egg of the fly with a very good microſcope; it is of a very pure red, perfectly tranſparent, except in the centre, where there were evident marks of the embryo forming, and opaque ramifications paſſing off from the body of it. The egg is perfectly oval, and about the ſize of an ant's egg. The maggot is about the one-eighth of an inch long, formed of many rings (ten or twelve) with a ſmall red head; when ſeen with a microſcope, the parts of the head were eaſily diſtinguiſhed, with ſix ſmall ſpecks on the breaſt, ſomewhat projecting, which ſeemed to be the incipient formation of the feet. This maggot is now in my cuſtody, in the form of a nymph or cryſalis, its annular coat forming a ſtrong covering, from which it ſhould iſſue forth a fly. I have never ſeen the fly, and cannot therefore deſcribe it more fully, or determine its genus and ſpecies. I am promiſed a drawing of the inſect in its different ſtages, and ſhall be able ſoon to add to a botanical deſcription of the plant a drawing of the branch, with the different parts of fructification and lac on it.

The gentleman to whom I owe part of my information terms the lac the excrement of the inſect. On a more minute inveſtigation, however, we may not find it more ſo than the wax or honey of the bee, or ſilk of the ſilk-worm. Nature has provided moſt inſects with the means of ſecreting a ſubſtance which generally anſwers the twofold purpoſe of defending the embryo, and ſupplying nouriſhment to the inſect from the time of its animation till able to wander abroad in queſt of food. The freſh lac contains within its cells a liquid, ſweetiſh to the taſte, and of a fine red colour, miſcible in water. The natives of Aſſam uſe it as a dye, and cotton dipped in this liquid makes afterwards a very good red ink.

The ſimple operation of purifying lac is practiſed as follows. It is broken into ſmall pieces, and picked from the branches and ſticks, when it is put into a ſort of canvas bag of about four feet long, and not above ſix inches in circumference. Two of theſe bags are in conſtant uſe, and each of them held by two men. The bag is placed over a fire, and frequently turned till the lac is liquid enough to paſs through its pores, when it is taken off the fire, and ſqueezed by two men in different directions, dragging it along the convex part of a plantain-tree prepared for the purpoſe; while this is doing, the other bag is heating, to be treated in the ſame way. The mucilaginous and ſmooth ſurface of the plantain-tree ſeems peculiarly well adapted for preventing the adheſion of the heated lac, and giving it the form which enhances its value ſo much. The degree of preſſure on the plantain-tree regulates the thickneſs of the ſhell, and the quality of the bag determines its fineneſs and tranſparency. They have learned of late, that the lac which is thicker in the ſhell than it uſed to be, is moſt prized in Europe. Aſſam furniſhes us with the greateſt quantity of lac in uſe; and it may not be generally known, that the tree on which they produce the beſt and largeſt quantity of lac is not uncommon in Bengal, and might be employed in propagating the fly, and cultivating the lac, to great advantage. The ſmall quantity of lac collected in theſe provinces affords a precarious and uncertain crop, becauſe not attended to. Some attention at particular ſeaſons is neceſſary to invite the fly to the tree; and collecting the whole of the lac with too great an avidity, where the inſect is not very generally to be met with, may annihilate the breed.

The beſt method of cultivating the tree, and preſerving the inſect, being properly underſtood in Bengal, would ſecure to the Coſs poſſeſſions the benefit ariſing from the ſale of a lucrative article, in great demand and of extenſive uſe.

Stages and diſtance from Rungpore to Taſſeſudon and Tiſſoolumboo, in computed coſſes and miles, two miles to a coſs.

		Coſſes.	M.	F.
1783,	From Rungpore to			
May 6.	Calamaty Plains	5½	11	0
8.	To Mongulhaut	5½	11	4
9.	To Belladinga	7	14	0
10.	To Bahar	4	8	0
11.	To Chichacotta in Boutan	13	26	0
12.	To Buxaduar	12	24	0
22.	To Joogagoo	5	10	0
23.	To Muriſhoug	5	10	0
25.	To Chooka	9	18	0
		66	132	4

		Coſſes.	M.	F.
	Brought over	66	132	4
1783, May 26.	To Punuka - -	7	14	0
27.	To Chepta - -	5	10	0
29.	To Pagha - -	5	10	0
30.	To Numloo - -	4	8	0
31.	To Wanakha - -	4	8	0
June 1.	To Taſſeſudon, capital of Boutan	3	6	0
		94	188	4
Sept. 8.	To Pimitung - -	7	14	0
9.	To Paraghon - -	6	12	0
11.	To Dukaigun - -	4	8	0
12.	To Sanha - -	5	10	0
13.	To a tent on Thibet ground -	8	16	0
14.	To Chichakumboo, Thibet -	4	8	0
15.	To Duina - -	10	20	0
16.	To Chalu - -	15	30	0
17.	To Simadar - -	9	18	0
18.	To Selu - - -	17	34	0
19.	To Takui - -	9	18	0
20.	To Dequini - -	14	28	0
21.	To Sehundi - -	15	30	0
22.	To Tiſſoolumboo, capital of Thibet	7	14	0
		224	448	4

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A
METEOROLOGICAL JOURNAL
KEPT AT THE APARTMENTS OF
THE ROYAL SOCIETY,
BY ORDER OF THE
PRESIDENT AND COUNCIL.

METEOROLOGICAL JOURNAL

for January 1788.

1788	Time.	Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H. M.	Deg.	Deg.	Inches.	Inch.	Points.	Str.	
Jan. 1	8 0	43	49	29,97		SSE	1	Cloudy.
	2 0	46	51	29,91		S	1	Fine.
2	8 0	48	49	29,58	0,040	S	2	Cloudy.
	2 0	48	52	29,24		S	2	Rainy.
3	8 0	42	50	28,97	0,235	S	2	Fair.
	2 0	46	52	28,89		S	2	Rainy.
4	8 0	43	51	29,13	0,024	SW by W	1	Fair.
	2 0	44	56	29,23		WSW	1	Fine.
5	8 0	37	52	29,35		SW	1	Fine.
	2 0	40	56	29,32		SW	1	Fine.
6	8 0	42	53	29,16		SW	1	Cloudy.
	2 0	42	56	29,37		SW	1	Cloudy.
7	8 0	37	52	29,66		WSW	1	Cloudy.
	2 0	41	54	29,78		W	1	Cloudy.
8	8 0	39	52	29,96		ENE	1	Cloudy.
	2 0	43	53	30,01		ENE	1	Cloudy.
9	8 0	40	51	30,18	0,020	E	2	Rainy.
	2 0	40	52	30,17		E	2	Cloudy.
10	8 0	37	51	30,21		ENE	1	Cloudy.
	2 0	40	53	30,21		NE	1	Cloudy.
11	8 0	37	51	30,32		NE	1	Cloudy.
	2 0	39	53	30,36		NE	1	Cloudy.
12	8 0	36	52	30,38		W	1	Foggy.
	2 0	39	53	30,36		WNW	1	Fine.
13	8 0	39	52	30,21		WNW	1	Cloudy.
	2 0	41	53	30,11		WNW	1	Cloudy.
14	8 0	36	52	29,98		W	1	Fine.
	2 0	41	53	30,19		W	2	Cloudy.
15	8 0	27	50	30,48		WNW	1	Fine.
	2 0	33	52	30,56		WNW	1	Fine.
16	8 0	26	50	30,68		WNW	1	Fine.
	2 0	36	52	30,70		WNW	1	Fine.

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	H.	M.	Deg.	Deg.	Inches.	Inch.	Points.	Str.	
Jan. 17	8	0	33	50	30,66		W	1	Foggy.
	2	0	40	53	30,63		W	1	Cloudy.
18	8	0	36	50	30,23		W	1	Cloudy.
	2	0	45	52	29,98		W	1	Cloudy.
19	8	0	37	50	29,68	0,060	WSW	2	Cloudy.
	2	0	43	53	29,85		WSW	2	Cloudy.
20	8	0	34	51	30,33		NW	1	Cloudy.
	2	0	40	53	30,38		WNW	1	Cloudy.
21	8	0	39	52	30,33		WNW	1	Cloudy.
	2	0	44	54	30,24		WSW	1	Cloudy.
22	8	0	37	52	29,97		W	1	Fine.
	2	0	44	54	30,00		WSW	2	Cloudy.
23	8	0	39	52	30,15		WNW	2	Cloudy.
	2	0	44	54	30,07		WNW	2	Cloudy.
24	8	0	44	52	29,86		W	2	Cloudy.
	2	0	48	54	29,84		W	2	Cloudy.
25	8	0	43	54	29,90	0,020	W	1	Cloudy.
	2	0	45	56	30,02		W	1	Fine.
26	8	0	39	55	30,06		W	1	Fine.
	2	0	46	55	29,98		WSW	1	Cloudy.
27	8	0	42	54	29,97	0,040	S by W	1	Cloudy.
	2	0	45	56	30,02		S	1	Cloudy.
28	8	0	38	54	30,27		SW	1	Cloudy.
	2	0	39	56	30,31		WSW	1	Cloudy.
29	8	0	32	53	30,33		WNW	1	Foggy.
	2	0	35	53	30,35		N	1	Foggy.
30	8	0	33	52	30,38		NE	1	Cloudy.
	2	0	38	53	30,41		NE	1	Cloudy.
31	8	0	36	51	30,30		E	1	Cloudy.
	2	0	38	54	30,21		ESE	1	Cloudy.

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1788	Time.		Therm. without	Therm within.	Barom.	Rain.	Winds.		Weather.
	H.	M.	Deg.	Deg.	Inches.	Inch.	Points.	Str.	
Feb. 1	8	0	32	50	30,00		ESE	1	Cloudy.
	2	0	33	50	29,87		ESE	1	Cloudy.
2	8	0	29	50	29,66		N	1	Cloudy.
	2	0	37	51	29,55		NW	2	Cloudy.
3	8	0	43	50	29,26	0,076	WSW	2	Cloudy.
	2	0	46	52	29,40		WSW	2	Cloudy.
4	8	0	35	50	29,73		SW	1	Foggy.
	2	0	42	53	29,77		SW	1	Fine.
5	8	0	40	50	29,70	0,112	E	1	Cloudy.
	2	0	46	52	29,71		E	1	Cloudy.
6	8	0	35	51	30,15		W	1	Foggy.
	2	0	45	53	30,18		W	1	Cloudy.
7	8	0	37	52	30,21		NE	1	Foggy.
	2	0	46	54	30,21		NE	1	Cloudy.
8	8	0	39	53	30,13		NE	1	Cloudy.
	2	0	38	53	29,98		NE	1	Rainy.
9	8	0	33	51	29,80	0,067	NNE	1	Cloudy.
	2	0	33	53	29,73		NNE	1	Cloudy.
10	8	0	33	51	29,73		N	1	Cloudy.
	2	0	35	51	29,77		NW	1	Cloudy.
11	8	0	38	50	29,89		SE	1	Cloudy.
	2	0	45	54	29,98		SSW	1	Cloudy.
12	8	0	40	51	30,21		SW	1	Foggy.
	2	0	48	52	30,16		SW	1	Cloudy.
13	8	0	43	52	29,94	0,040	SW	1	Small rain.
	2	0	49	54	29,97		W	1	Fair.
14	8	0	40	52	30,12		W	1	Cloudy.
	2	0	49	54	30,06		W	1	Cloudy.
15	8	0	44	53	29,86	0,146	WNW	1	Cloudy.
	2	0	50	56	29,85		NW	1	Fine.
16	8	0	41	54	29,76	0,053	WSW	1	Rain.
	2	0	47	56	29,65		WSW	1	Fair.

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1788	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.	Deg.	Deg.	Inches.	Inch.	Points.	Str.	
Feb. 17	8	0	46	56	29,71		W	1	Cloudy.
	2	0	47	56	29,75		SSW	1	Cloudy.
18	8	0	33	53	29,83	0,132	E	1	Cloudy.
	2	0	39	53	29,79		ESE	1	Fair.
19	8	0	39	52	29,45		E	1	Cloudy.
	2	0	44	54	29,33		SSE	1	Fine.
20	8	0	39	51	29,02	0,032	E	1	Cloudy.
	2	0	42	54	28,91		E	1	Cloudy.
21	8	0	41	52	28,71	0,235	E	1	Rainy.
	2	0	47	54	28,65		SSE	1	Rainy.
22	8	0	44	53	28,76	0,325	SW	1	Cloudy.
	2	0	47	54	28,94		WSW	1	Cloudy.
23	8	0	41	53	29,05		SW	1	Fine.
	2	0	48	54	29,08		SSW	1	Fair.
24	8	0	42	53	28,98		ESE	1	Cloudy.
	2	0	44	54	28,91		NE	1	Rain.
25	8	0	39	53	29,33	0,168	NW	1	Cloudy.
	2	0	43	54	29,50		WNW	1	Cloudy.
26	8	0	30	52	29,62		WNW	1	Fine.
	2	0	45	54	29,56		W	1	Fair.
27	8	0	41	52	29,31	0,038	E	1	Rain.
	2	0	44	54	29,35		E	1	Cloudy.
28	8	0	41	52	29,50		E	1	Cloudy.
	2	0	44	53	29,47		E	1	Cloudy.
29	8	0	42	53	29,28	0,037	SE	1	Cloudy.
	2	0	48	54	29,20		SSE	1	Cloudy.

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1788	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.	Deg.	Deg.	Inches.	Inch.	Points.	Str.	
Mar. 1	7	0	35	53	29,43		SW by S	1	Fine.
	2	0	45	54	29,48		SSW	1	Fair.
2	7	0	41	52	29,52		E	1	Cloudy.
	2	0	42	53	29,63		ESE	2	Cloudy.
3	7	0	35	51	29,99		E	2	Cloudy.
	2	0	41	53	30,08		E	2	Fair.
4	7	0	36	51	30,03		NW	1	Foggy.
	2	0	42	53	29,92		W	1	Fair.
5	7	0	33	51	29,97		NNW	1	Cloudy.
	2	0	41	53	29,88		W	1	Fair.
6	7	0	39	51	29,34	0,075	WSW	1	Fair.
	2	0	43	53	29,45		WNW	2	Cloudy.
7	7	0	34	50	29,43		NW	1	Fair.
	2	0	40	51	29,51		NW	1	Fair.
8	7	0	28	48	29,52		NE	1	Fair.
	2	0	39	50	29,54		NE	1	Fair.
9	7	0	29	48	29,59		NE	2	Fair.
	2	0	39	49	29,64		ENE	2	Fair.
10	7	0	30	47	29,89		ENE	1	Fine.
	2	0	38	50	29,97		ENE	1	Fair.
11	7	0	28	46	30,06		ENE	1	Fine.
	2	0	38	50	30,08		ENE	1	Fine.
12	7	0	29	47	30,03		E	1	Fine.
	2	0	39	51	30,00		ENE	1	Fine.
13	7	0	29	47	29,84		ESE	1	Cloudy.
	2	0	32	48	29,73		ESE	1	Cloudy.
14	7	0	31	46	29,52		E	1	Fine.
	2	0	39	49	29,44		ESE	1	Fair.
15	7	0	36	46	29,44		ESE	1	Cloudy.
	2	0	40	49	29,46		ESE	1	Cloudy.
16	7	0	38	47	29,46	0,120	ESE	1	Cloudy.
	2	0	37	47	29,46		ESE	2	Cloudy.

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1788	Time.		Therm without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.	Deg.	Deg.	Inches.	Inch.	Points.	Str.	
Mar. 17	7	0	33	46	29,50		ESE	2	Cloudy.
	2	0	33	47	29,52		ESE	2	Cloudy.
18	7	0	33	46	29,68		ESE	2	Cloudy.
	2	0	39	49	29,75		ESE	2	Cloudy.
19	7	0	36	47	30,00		SE by E	1	Fair.
	2	0	42	49	30,03		SE by E	1	Fair.
20	7	0	41	47	29,82		SE	1	Cloudy.
	2	0	50	50	29,74		SW	1	Fair.
21	7	0	44	49	29,76	0,070	SSW	1	Fair.
	2	0	52	53	29,72		SW	1	Fair.
22	7	0	43	50	29,57		SSW	1	Cloudy.
	2	0	47	53	29,48		SSE	1	Cloudy.
23	7	0	39	50	29,32	0,071	N	2	Cloudy,
	2	0	44	53	29,47		N	2	Cloudy.
24	7	0	40	49	29,61		SW	2	Fair.
	2	0	51	53	29,59		SSW	2	Hazy.
25	7	0	46	51	29,52		SSW	2	Cloudy,
	2	0	52	54	29,58		SSW	2	Fair.
26	7	0	40	52	29,60		SSE	2	Fine.
	2	0	53	55	29,52		SSE	2	Fine.
27	7	0	42	54	29,52		S	1	Fine.
	2	0	51	57	29,44		SSE	2	Fine.
28	7	0	46	55	29,45		SSW	2	Fair.
	2	0	56	57	29,57		SW	2	Fair.
29	7	0	46	55	29,92		WSW	1	Cloudy.
	2	0	56	58	30,01		W	1	Fair.
30	7	0	50	56	29,95		WSW	1	Cloudy.
	2	0	59	58	29,90		SW	2	Fine.
31	7	0	48	58	29,63		W	2	Fine.
	2	0	49	59	29,68		W	2	Fair.

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1788	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.	Deg.	Deg.	Inches.	Inch.	Points.	Str.	
April 1	7	0	47	56	29,72	0,390	W	2	Rainy.
	2	0	55	60	29,74		WNW	2	Fair.
2	7	0	46	57	30,09		W	2	Fair.
	2	0	58	60	30,02		W	2	Fair.
3	7	0	47	58	29,72		W	2	Fair.
	2	0	54	60	29,50		W	2	Fair.
4	7	0	43	57	29,67		NW	2	Cloudy.
	2	0	40	58	29,75		NW by N	2	Hail.
5	7	0	40	55	29,97		NNW	2	Cloudy.
	2	0	41	57	30,10		N	2	Cloudy.
6	7	0	42	55	30,22	0,020	W	1	Fair.
	2	0	51	56	30,23		W	1	Cloudy.
7	7	0	48	55	30,27		W	1	Cloudy.
	2	0	51	56	30,30		WNW	1	Cloudy.
8	7	0	51	56	30,39		WNW	1	Cloudy.
	2	0	59	59	30,42		WNW	1	Cloudy.
9	7	0	48	56	30,48		WNW	1	Fair.
	2	0	59	59	30,47		NNW	1	Cloudy.
10	7	0	51	57	30,40		E	1	Cloudy.
	2	0	59	59	30,30		ESE	1	Fine.
11	7	0	49	57	30,16		E	1	Fine.
	2	0	58	59	30,07		SE	1	Fine.
12	7	0	48	57	29,97		SSW	1	Fine.
	2	0	56	58	30,09		SSW	1	Fine.
13	7	0	45	57	30,28		SW	1	Fine.
	2	0	59	61	30,25		SW by W	1	Fine.
14	7	0	45	58	30,06		W	1	Fine.
	2	0	53	61	30,00		W	1	Rainy.
15	7	0	43	57	30,03	0,097	W	1	Fine.
	2	0	51	59	30,06		W	1	Fair.
16	7	0	45	57	30,11		W	1	Fair.
	2	0	53	59	30,11		W	1	Fair.

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1788	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.	Deg.	Deg.	Inches.	Inch.	Points.	Str.	
Apr. 17	7	0	48	58	30,09		W	1	Cloudy.
	2	0	55	60	30,09		W	1	Cloudy.
18	7	0	50	59	30,21		W	1	Fair.
	2	0	62	61	30,26		N	1	Fair.
19	7	0	54	60	30,27		E	1	Fair.
	2	0	65	61	30,24		SE	1	Fine.
20	7	0	52	60	30,03		SSE	1	Fine.
	2	0	67	61	29,88		SSW	1	Fine.
21	7	0	45	59	29,76	0,100	SW	1	Fine.
	2	0	56	60	29,70		WSW	2	Fine.
22	7	0	46	59	29,95		SW	2	Fine.
	2	0	55	61	29,90		SW	1	Cloudy.
23	7	0	53	60	29,91		WSW	1	Cloudy.
	2	0	63	61	29,89		W	1	Cloudy.
24	7	0	47	60	29,95		W	1	Fine.
	2	0	56	60	30,01		WNW	2	Fine.
25	7	0	49	58	29,97		WNW	2	Fair.
	2	0	57	61	29,96		WNW	2	Fair.
26	7	0	54	59	30,15		W	1	Cloudy.
	2	0	61	60	30,18		W by S	2	Fair.
27	7	0	48	58	30,28		SW	1	Fine.
	2	0	63	61	30,28		WSW	1	Fine.
28	7	0	47	59	30,32		W	1	Fine.
	2	0	66	60	30,28		SE	1	Fine.
29	7	0	55	60	30,27		ESE	1	Fine.
	2	0	68	63	30,22		ESE	1	Fine.
30	7	0	55	60	30,22		ESE	1	Fine.
	2	0	68	63	30,20		ESE	1	Fine.

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1788	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.	Deg.	Deg.	Inches.	Inch.	Points.	Str.	
May 1	7	0	54	60	30,18		ESE	1	Fine.
	2	0	69	63	30,16		ESE	1	Fine.
2	7	0	56	61	30,14		NE	1	Fine.
	2	0	71	64	30,14		E	2	Fine.
3	7	0	50	60	30,34		ENE	2	Cloudy.
	2	0	57	62	30,32		ENE	2	Fair.
4	7	0	50	60	30,29		NE	1	Cloudy.
	2	0	55	61	30,27		NNE	2	Fair.
5	7	0	51	60	30,22		ESE	2	Fine.
	2	0	64	61	30,14		ESE	2	Fine.
6	7	0	54	61	30,02		ESE	1	Fine.
	2	0	68	62	29,98		ESE	1	Fine.
7	7	0	57	61	29,88		SSE	1	Fair.
	2	0	64	63	29,86		SW by S	1	Cloudy.
8	7	0	54	61	29,96	0,025	SSW	1	Fine.
	2	0	66	64	29,93		SSW	1	Fine.
9	7	0	53	62	29,91		SSW	1	Cloudy.
	2	0	64	64	29,86		SW	2	Fair.
10	7	0	53	61	29,93	0,080	WSW	2	Fine.
	2	0	62	63	29,98		W	1	Fine.
11	7	0	49	60	30,18		WNW	1	Cloudy.
	2	0	62	62	30,17		WSW	1	Fair.
12	7	0	55	60	30,27		WSW	1	Fair.
	2	0	64	62	30,26		SSE	1	Fair.
13	7	0	51	60	30,31		E by S	1	Fine.
	2	0	59	62	30,28		ESE	1	Fine.
14	7	0	52	60	30,18		NE	1	Hazy.
	2	0	56	59	30,16		E	2	Cloudy.
15	7	0	52	59	30,12		ENE	2	Fair.
	2	0	58	60	30,05		ENE	2	Cloudy.
16	7	0	50	59	29,85	0,030	E	2	Rainy.
	2	0	61	60	29,92		E	2	Fine.

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1788	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.	Deg.	Deg.	Inches.	Inch.	Points.	Str.	
May 17	7	0	54	60	29,96	0,031	E	2	Fair.
	2	0	68	61	29,91		ESE	2	Fair.
18	7	0	55	59	29,87		E	1	Cloudy.
	2	0	59	61	29,87				Rainy.
19	7	0	54	60	29,89	0,260	NE	1	Cloudy.
	2	0	61	60	29,93		NNE	1	Cloudy.
20	7	0	55	60	30,13		NNE	1	Fine.
	2	0	70	61	30,15		N	1	Fine.
21	7	0	55	60	30,28		NW	1	Fine.
	2	0	70	61	30,28		WSW	1	Fine.
22	7	0	56	61	30,28		SW	1	Fine.
	2	0	68	63	30,27		WSW	1	Fine.
23	7	0	57	63	30,20		SW	1	Cloudy.
	2	0	68	63	30,16		SW	1	Cloudy.
24	7	0	57	63	30,08		SW	1	Fine.
	2	0	75	66	30,02		SSW	1	Fine.
25	7	0	63	65	30,00		S	1	Fine.
	2	0	75	69	30,02		SSW	2	Fine.
26	7	0	63	67	30,00		SE by S	1	Fine.
	2	0	73	72	30,00		SSE	1	Fine.
27	7	0	63	70	29,91		SE	1	Fine.
	2	0	80	73	29,85		SE	1	Fine.
28	7	0	67	70	29,71		E	1	Fine.
	2	0	73	71	29,66		SSE	1	Hazy.
29	7	0	63	70	29,58	0,071	SW	1	Cloudy.
	2	0	61	68	29,67			0	Cloudy.
30	7	0	52	66	29,71		E	1	Cloudy.
	2	0	55	65	29,72		E	1	Cloudy.
31	7	0	52	64	29,88		NNE	1	Cloudy.
	2	0	58	63	29,94		NE	1	Cloudy.

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1788	Time.	Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H. M.	Deg.	Deg.	Inches.	Inch.	Points.	Str.	
June 1	7 0	52	62	30,02		E	1	Fair.
	2 0	64	63	29,99		E	1	Cloudy.
2	7 0	54	62	29,98		SSE	1	Fine.
	2 0	68	63	29,98		SSE	1	Fine.
3	7 0	56	62	30,02		E	1	Fair.
	2 0	67	63	30,02		E	1	Fine.
4	7 0	57	63	30,00		WNW	1	Fine.
	2 0	62	63	30,04		WNW	1	Cloudy.
5	7 0	52	62	30,22			0	Fine.
	2 0	68	63	30,28		W	1	Fine.
6	7 0	60	63	30,20		W	1	Fine.
	2 0	72	66	30,08		SW	1	Fine.
7	7 0	60	65	30,00		W	1	Fair.
	2 0	72	66	29,97		W	1	Fair.
8	7 0	56	61	30,08	0,035	ESE	1	Fine.
	2 0	62	63	30,10		ESE	1	Fine.
9	7 0	52	62	30,17		ENE	2	Cloudy.
	2 0	64	63	30,19		NE	2	Fine.
10	7 0	55	62	30,21		N	2	Fine.
	2 0	69	64	30,18		ESE	2	Fine.
11	7 0	55	63	30,15		NE	1	Fine.
	2 0	67	64	30,08		NE	1	Fine.
12	7 0	54	63	30,06		NNE	1	Fine.
	2 0	70	66	30,04		NE	1	Fine.
13	7 0	57	63	30,06		NE	2	Fine.
	2 0	70	66	30,06		NNE	2	Fine.
14	7 0	58	64	30,07		NNE	1	Cloudy.
	2 0	67	65	30,06		NNE	1	Cloudy.
15	7 0	59	65	30,05		NNE	1	Fine.
	2 0	72	66	30,03		N	1	Fine.
16	7 0	59	59	30,00	0,120	N	1	Rain.
	2 0	68	66	29,96		N	1	Fine.

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1788	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.	Deg.	Deg.	Inches.	Inch.	Points.	Str.	
June 17	7	0	60	65	29,99		N	1	Fine.
	2	0	80	67	29,95		N	1	Fine.
18	7	0	65	69	29,87		E	1	Cloudy.
	2	0	79	70	29,84		SSE	1	Cloudy.
19	7	0	63	69	29,81	0,023	SSE	1	Cloudy.
	2	0	66	68	29,87		S	1	Rain.
20	7	0	56	67	29,94	0,060	SE	1	Cloudy.
	2	0	60	67	29,94		SE	1	Rain.
21	7	0	60	66	29,98	0,030	NNE	1	Cloudy.
	2	0	60	67	29,98		N	1	Cloudy.
22	7	0	63	66	29,98		NNW	1	Fair.
	2	0	72	68	29,96		NNW	1	Fair.
23	7	0	62	66	29,80		W	1	Fine.
	2	0	65	67	29,75		SSW	1	Cloudy.
24	7	0	58	66	29,70		SSW	2	Fair.
	2	0	63	66	29,68		SSW	2	Cloudy.
25	7	0	58	66	29,63	0,245	SW	1	Fair.
	2	0	65	67	29,60		SW	1	Cloudy.
26	7	0	58	66	29,61	0,050	WSW	1	Cloudy.
	2	0	63	66	29,57		SW by S	1	Cloudy, heavy rain with thund.
27	7	0	58	65	29,53	2,116	SW by S	1	Fair.
	2	0	71	67	29,49		S	1	Fair.
28	7	0	59	66	29,54	0,285	SSE	1	Rain.
	2	0	63	66	29,54		SE	1	Rain.
29	7	0	57	65	29,73	0,311	SE	1	Cloudy.
	2	0	64	65	29,79		SE	1	Cloudy.
30	7	0	59	64	29,86		W	1	Cloudy.
	2	0	67	65	29,93		W	1	Cloudy.

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1788	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.	Deg.	Deg.	Inches.	Inch.	Points.	Sts.	
July 1	7	0	60	64	30,07		W	2	Cloudy.
	2	0	63	64	30,07		W	2	Cloudy.
2	7	0	61	64	30,07		SW	2	Fair.
	2	0	69	65	30,07		SW	2	Fair.
3	7	0	63	65	30,04		S	1	Fair.
	2	0	73	66	29,93		SSW	1	Fine.
4	7	0	58	65	29,74		SSW	2	Rain.
	2	0	70	66	29,77		SSW	2	Fine.
5	7	0	57	65	29,77	0,040	SSW	1	Fine.
	2	0	67	66	29,73		SSW	1	Fine.
6	7	0	57	64	29,81		SW	1	Fine.
	2	0	65	65	29,89		SW	1	Fair.
7	7	0	56	64	29,85		SSW	2	Fair.
	2	0	63	65	29,84		WSW	2	Rain.
8	7	0	57	65	29,90	0,250	SW	1	Fair.
	2	0	62	65	29,80		SSW	1	Cloudy.
9	7	0	56	64	29,76	0,210	SSW	1	Fair.
	2	0	62	65	29,76		S	1	Cloudy.
10	7	0	61	65	29,81		SSW	2	Cloudy.
	2	0	70	66	29,82		SSW	2	Cloudy.
11	7	0	62	65	29,90		S	2	Cloudy.
	2	0	68	66	29,92		S	1	Cloudy.
12	7	0	64	66	29,93	0,250	S	1	Rain.
	2	0	75	67	29,77		SSW	1	Fair.
13	7	0	61	66	29,73		SSW	2	Cloudy.
	2	0	67	66	29,78		SSW	2	Cloudy.
14	7	0	58	66	29,87	0,132	SSW	1	Fair.
	2	0	64	67	29,89		S	1	Fair.
15	7	0	58	66	29,82	0,111	SSW	2	Rain.
	2	0	67	68	29,77		SSW	2	Cloudy.
16	7	0	57	66	29,75	0,082	SSW	2	Cloudy.
	2	0	64	67	29,78		SSW	2	Cloudy.

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1788	Time.		Therm without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.	Deg.	Deg.	Inches.	Inch.	Points.	Str.	
July 17	7	0	57	66	29,83		SW	2	Rain.
	2	0	67	67	29,85		W	1	Fair.
18	7	0	57	66	30,15	0,480	W	1	Fine.
	2	0	71	67	30,16		W	1	Fine.
19	7	0	62	66	30,14		SSW	2	Fine.
	2	0	69	68	30,08		SSW	2	Cloudy.
20	7	0	59	66	30,05		WSW	1	Cloudy.
	2	0	68	67	30,13		W	1	Cloudy.
21	7	0	60	66	30,20	0,065	ESE	1	Fair.
	2	0	67	66	30,22		NW	1	Cloudy.
22	7	0	60	66	30,21		W	1	Fair.
	2	0	71	67	30,20		W	1	Fine.
23	7	0	61	66	30,17		WNW	1	Hazy.
	2	0	71	67	30,11		W	1	Cloudy.
24	7	0	60	66	30,05		W	1	Fine.
	2	0	67	67	30,00		W	1	Cloudy,
25	7	0	60	66	30,11		W	1	Fair.
	2	0	65	67	30,10		W	1	Fair.
26	7	0	55	65	30,15		W	1	Cloudy.
	2	0	67	66	30,15		W	1	Cloudy.
27	7	0	55	65	30,14		W	1	Fine.
	2	0	69	66	30,12		W	1	Cloudy.
28	7	0	60	65	30,10		W	1	Fair.
	2	0	71	66	30,10		W	1	Fine.
29	7	0	59	65	30,11		WNW	1	Fair.
	2	0	72	66	30,10		NW	1	Fine.
30	7	0	62	66	30,16		SE by S	1	Cloudy.
	2	0	76	68	30,17		SW	1	Fine.
31	7	0	60	66	30,22		SW	1	Fine.
	2	0	77	69	30,20		SW	1	Fine.

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1788	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.	Deg.	Deg.	Inches.	Inch.	Points.	Str.	
Aug. 1	7	0	65	68	30,23		SW	1	Cloudy.
	2	0	72	69	30,29		W	1	Cloudy.
2	7	0	64	68	30,45		N	1	Fine.
	2	0	73	70	30,44		NE	1	Fine.
3	7	0	65	68	30,43		E	1	Fine.
	2	0	76	70	30,43		E	1	Fine.
4	7	0	60	66	30,43		ESE	1	Hazy.
	2	0	77	71	30,37				Fine.
5	7	0	61	67	30,30		NE	1	Hazy.
	2	0	69	69	30,25		N	1	Fair.
6	7	0	57	67	30,21		NNE	1	Fair.
	2	0	64	67	30,18		N	2	Cloudy.
7	7	0	58	66	30,12		NNE	1	Cloudy.
	2	0	62	66	30,12		NNE	1	Cloudy.
8	7	0	58	66	30,19		NNE	1	Cloudy.
	2	0	62	66	30,19		NNE	1	Cloudy.
9	7	0	58	66	30,19		NE	1	Clondy.
	2	0	66	66	30,18		ENE	1	Cloudy.
10	7	0	58	65	30,14		NE	1	Cloudy.
	2	0	66	66	30,11		NE	1	Cloudy.
11	7	0	58	65	30,08		ENE	1	Cloudy.
	2	0	68	65	30,04		ENE	1	Cloudy.
12	7	0	57	65	29,95			0	Fair.
	2	0	71	66	29,83		SSW	1	Cloudy.
13	7	0	60	66	29,58	0,325	W	1	Cloudy.
	2	0	70	67	29,51		SW	1	Cloudy.
14	7	0	58	66	29,22	0,293	SW	1	Rain.
	2	0	66	66	29,37		SW	2	Rain.
15	7	0	57	65	29,60	0,230	WSW	2	Fine.
	2	0	67	66	29,60		WSW	2	Fair.
16	7	0	57	65	29,74	0,115	WSW	2	Fine.
	2	0	57	65	29,74		SW	1	Rain.

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for Auguſt 1788.

1788	Time.		Therm without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.	Deg.	Deg.	Inches.	Inch.	Points.	Str.	
Aug. 17	7	0	58	65	29,72	0,673	S by W	1	Rain.
	2	0	61	65	29,64		S by W	1	Fair.
18	7	0	57	65	29,80	0,440	WSW	1	Fine.
	2	0	63	65	29,84		WSW	1	Rain.
19	7	0	59	65	29,68	0,130	SSW	2	Cloudy.
	2	0	65	66	29,66		SSW	2	Cloudy.
20	7	0	57	65	29,79	0,080	WSW	1	Fair.
	2	0	71	66	29,85		WSW	1	Fair.
21	7	0	57	66	29,99	0,023	SSW	1	Fair.
	2	0	70	66	29,98		SSW	1	Fair.
22	7	0	59	66	29,76	0,080	SSE	1	Rain.
	2	0	64	66	29,67		S	1	Rain.
23	7	0	59	66	29,67	0,045	SW	1	Cloudy.
	2	0	70	66	29,72		SW	1	Fine.
24	7	0	56	66	29,95		WSW	1	Fine.
	2	0	67	66	29,97		SW	1	Cloudy.
25	7	0	53	65	29,97	0,012	SW	1	Fine.
	2	0	69	66	29,94		SW	1	Fine.
26	7	0	57	65	29,82		SE	1	Cloudy.
	2	0	68	67	29,82		N	1	Fine.
27	7	0	57	65	29,93		W	1	Cloudy.
	2	0	63	65	29,96		W	1	Cloudy.
28	7	0	55	64	30,03		W	2	Cloudy.
	2	0	70	66	30,03		W	1	Fine.
29	7	0	58	65	29,97		SW	1	Cloudy.
	2	0	58	65	29,82		SSW	1	Rain.
30	7	0	55	64	29,82	0,230	W	2	Fine.
	2	0	67	65	29,84		W	1	Cloudy,
31	7	0	57	64	29,80	0,023	SW by S	1	Cloudy.
	2	0	62	64	29,74		SSW	1	Cloudy.

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1788	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.	Deg.	Deg.	Inches.	Inch.	Points.	Str.	
Sept. 1	7	0	59	64	29,87	0,220	NW	1	Fine.
	2	0	68	65	29,91		W	1	Cloudy.
2	7	0	58	64	29,98		WSW	1	Cloudy.
	2	0	67	65	29,98		WSW	1	Cloudy.
3	7	0	59	64	30,00		SW	1	Cloudy.
	2	0	67	65	30,00		SSW	1	Fair.
4	7	0	59	65	29,75		SE	1	Fine.
	2	0	74	67	29,71		SE	2	Fine.
5	7	0	60	66	29,78		SSE	1	Fair.
	2	0	70	67	29,80		SE	1	Cloudy.
6	7	0	55	66	29,90	0,905	SW	1	Fair.
	2	0	63	66	29,94		SSW	1	Cloudy.
7	7	0	55	66	30,15		SSE	1	Fine.
	2	0	72	67	30,17		SSE	1	Fine.
8	7	0	55	66	30,14		SSW	1	Fine.
	2	0	67	67	30,10		SW	1	Cloudy.
9	7	0	57	65	30,08		WSW	1	Cloudy.
	2	0	69	66	30,08		W	1	Cloudy.
10	7	0	53	65	30,07		W	1	Foggy.
	2	0	67	67	30,07		W	1	Fine.
11	7	0	58	66	30,08		SW	1	Cloudy.
	2	0	63	66	30,16		SSE	1	Fair.
12	7	0	51	65	30,25	0,070	ESE	1	Foggy.
	2	0	67	66	30,18		ESE	1	Fine.
13	7	0	56	65	29,98		ESE	1	Fine.
	2	0	67	66	29,88		W	1	Cloudy.
14	7	0	54	65	29,94		W	1	Fine.
	2	0	62	66	30,01		NNE	1	Fine.
15	7	0	50	64	30,12		NNE	1	Fine.
	2	0	61	64	30,05		E	1	Cloudy.
16	7	0	55	63	29,82		E	1	Cloudy.
	2	0	67	64	29,79		SSE	1	Fair.

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1788	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.	Deg.	Deg.	Inches.	Inch.	Points.	Str.	
Sept. 17	7	0	50	63	29,88	0,067	W	1	Fine.
	2	0	62	65	29,87		E	1	Fine.
18	7	0	56	64	29,49	0,105	ESE	2	Rain.
	2	0	62	64	29,50		E	2	Cloudy.
19	7	0	56	63	29,55	0,513	E	1	Rain.
	2	0	60	64	29,60		ESE	1	Cloudy.
20	7	0	54	63	29,57	0,280	ESE	1	Cloudy.
	2	0	60	64	29,47			0	Cloudy.
21	7	0	53	63	29,37	0,300		0	Cloudy.
	2	0	58	63	29,44		W	1	Cloudy.
22	7	0	45	60	29,55	0,128	W	1	Fine.
	2	0	53	60	29,50		SW	2	Cloudy.
23	7	0	52	61	29,75	0,145	W by S	1	Cloudy.
	2	0	60	62	29,74		SW	1	Cloudy.
24	7	0	52	61	29,67	0,302	W	1	Rain.
	2	0	58	61	29,68		SW	1	Cloudy.
25	7	0	45	59	29,81	0,062	W	1	Fine.
	2	0	59	60	29,88		WSW	1	Fair.
26	7	0	57	60	29,83		SSW	2	Cloudy.
	2	0	65	61	29,84		SSW	2	Fair.
27	7	0	52	60	29,95	0,058	SSW	2	Fine.
	2	0	61	62	30,00		SSW	2	Fine.
28	7	0	48	60	30,03		SW	1	Fine.
	2	0	60	62	29,90		SSW	2	Cloudy.
29	7	0	52	61	29,56	0,190	W	2	Fine.
	2	0	57	62	29,61		W	2	Fine.
30	7	0	50	60	29,86		W	2	Fine.
	2	0	56	60	29,92		W	2	Cloudy.

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1788	Time.	Therm without	Therm within.	Barom.	Rain	Winds.		Weather.
	H. M.	Deg.	Deg.	Inches.	Inch.	Points.	Str.	
Oct. 1	7 0	53	59	29,92		W	2	Cloudy.
	2 0	58	60	29,90		W	1	Rain.
2	7 0	59	60	29,94	0,035	W	2	Cloudy.
	2 0	67	62	29,94		W	2	Cloudy.
3	7 0	57	61	29,98		W	2	Cloudy.
	2 0	63	62	30,02		W	2	Cloudy.
4	7 0	56	61	30,08		W	1	Fine.
	2 0	62	62	30,09		W	1	Cloudy.
5	7 0	56	61	30,00		SSW	1	Cloudy.
	2 0	58	62	29,95		SW	2	Cloudy.
6	7 0	55	61	29,70		SW	2	Cloudy.
	2 0	53	61	29,65		SW	2	Rain.
7	7 0	46	59	30,14	0,068	SSW	1	Rain.
	2 0	52	60	30,28		SSE	2	Fine.
8	7 0	43	58	30,55		SE by S	2	Fine.
	2 0	55	58	30,55		NE.	2	Cloudy.
9	7 0	50	57	30,50		ENE	2	Fine.
	2 0	57	58	30,47		E	2	Fine.
10	7 0	47	57	30,44		E	2	Fine.
	2 0	59	59	30,44		E	2	Fine.
11	7 0	50	57	30,38		SE	2	Cloudy.
	2 0	59	61	30,35		E	2	Cloudy.
12	7 0	51	59	30,32		NE	2	Cloudy.
	2 0	59	61	30,32		NE	1	Fine.
13	7 0	51	60	30,23		NE	1	Cloudy.
	2 0	58	62	30,26		NE	1	Fine.
14	7 0	51	61	30,25		NE	1	Cloudy.
	2 0	56	62	30,22		NE	1	Cloudy.
15	7 0	42	60	30,05		NE	1	Fine.
	2 0	56	62	29,90		E	1	Fine.
16	7 0	42	60	29,66		NW	1	Foggy.
	2 0	52	60	29,64		W	1	Fair.

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1788	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.	Deg.	Deg.	Inches.	Inch.	Points.	Str.	
Oct. 17	7	0	50	59	29,74		W	1	Foggy.
	2	0	56	61	29,83		W	1	Cloudy.
18	7	0	38	57	30,15		N	1	Fine.
	2	0	50	60	30,22		NNE	1	Fine.
19	7	0	33	57	30,36		NNE	1	Foggy.
	2	0	43	57	30,36		NNE	1	Hazy.
20	7	0	36	54	30,32			0	Foggy.
	2	0	50	59	30,28		SW	1	Fine.
21	7	0	50	56	30,12		SW	1	Cloudy.
	2	0	60	60	30,06		WSW	1	Cloudy.
22	7	0	49	57	30,25		WSW	1	Cloudy.
	2	0	58	60	30,30		WSW	1	Cloudy.
23	7	0	53	58	30,27		WSW	1	Cloudy.
	2	0	60	62	30,20		W	1	Fair.
24	7	0	49	58	30,12		W	1	Cloudy.
	2	0	56	60	30,10		W by N	1	Fine.
25	7	0	44	58	30,11		WNW	1	Fair.
	2	0	53	60	30,15		NNW	1	Cloudy.
26	7	0	41	58	30,19		WNW	1	Fair.
	2	0	54	60	30,15		WNW	1	Fair.
27	7	0	47	58	30,08		WNW	1	Foggy.
	2	0	53	60	30,06		WNW	1	Cloudy.
28	7	0	42	58	30,02		WNW	1	Cloudy.
	2	0	52	60	30,00		WNW	1	Cloudy.
29	7	0	41	58	30,05			0	Foggy.
	2	0	48	60	30,06			0	Foggy.
30	7	0	42	58	30,15		N	1	Hazy.
	2	0	51	59	30,22		NE	1	Cloudy.
31	7	0	45	57	30,43		N	1	Cloudy.
	2	0	52	59	30,47		E	1	Cloudy.

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1788	Time.		Therm. without	Therm within.	Barom.	Rain.	Winds.		Weather.
	H.	M.	Deg.	Deg.	Inches.	Inch.	Points.	Str.	
Nov. 1	7	0	41	57	30,50		E	1	Foggy.
	2	0	51	60	30,43		E	1	Fine.
2	7	0	43	57	30,18		E	1	Foggy.
	2	0	55	61	30,08		S	1	Fine.
3	7	0	55	59	29,92		SSW	2	Cloudy.
	2	0	58	61	29,77		SSW	2	Cloudy.
4	7	0	48	60	29,61	0,190	SW	2	Fine.
	2	0	55	61	29,81		WSW	2	Fair.
5	7	0	38	58	30,13		W	1	Fair.
	2	0	47	61	30,18		W	1	Fine.
6	7	0	37	57	30,29			0	Foggy.
	2	0	48	59	30,26		SSE	1	Fine.
7	7	0	43	57	30,05		SE	2	Fair.
	2	0	49	59	29,94		SSE	2	Cloudy.
8	7	0	49	58	29,94		W	1	Cloudy.
	2	0	50	59	29,97		NW	1	Cloudy.
9	7	0	41	58	29,91		E	1	Foggy.
	2	0	53	59	29,77		SSE	1	Fair.
10	7	0	44	57	29,73		E	1	Fair.
	2	0	53	60	29,84		E	1	Fair.
11	7	0	43	59	30,05		WSW	1	Fair.
	2	0	53	59	30,12		WSW	1	Fair.
12	7	0	47	59	30,20		WSW	1	Fair.
	2	0	55	60	30,18		WSW	1	Cloudy.
13	7	0	51	59	29,85		SW	2	Cloudy.
	2	0	54	60	29,74		SSW	1	Cloudy.
14	7	0	36	58	30,08	0,200	W	1	Fine.
	2	0	42	59	30,10		WNW	1	Fine.
15	7	0	35	56	30,01	0,120	WNW	1	Cloudy.
	2	0	40	58	30,11		NW	1	Fine.
16	7	0	32	54	30,43		NNW	1	Fine.
	2	0	38	57	30,42		N	1	Fine.

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1788	Time.		Therm. without	Therm within.	Barom.	Rain.	Winds.		Weather.
	H	M.	Deg.	Deg.	inches.	Inch.	Points	Str.	
Nov. 17	7	0	42	54	30,17		WNW	1	Fair.
	2	0	48	57	30,17		WNW	1	Fine.
18	7	0	38	54	30,17		WNW	1	Fine.
	2	0	45	57	30,22		WNW	1	Fine.
19	7	0	42	55	30,21				Foggy.
	2	0	48	57	30,13		WNW	1	Cloudy.
20	7	0	42	54	30,20				Foggy.
	2	0	45	57	30,20				Foggy.
21	7	0	45	54	30,26				Foggy.
	2	0	49	57	30,25		W	1	Cloudy.
22	7	0	47	56	30,25		SW	1	Cloudy.
	2	0	49	58	30,25		SSW	1	Cloudy.
23	7	0	39	55	30,20		ESE	1	Fine.
	2	0	47	58	30,15		ESE	1	Fine.
24	7	0	36	55	30,11		E	1	Fine.
	2	0	43	58	30,15		E	1	Fine.
25	7	0	38	54	30,22		E	1	Cloudy.
	2	0	38	58	30,28		ENE	1	Fine.
26	7	0	29	52	30,34		ENE	1	Fine.
	2	0	35	54	30,29		ENE	1	Cloudy.
27	7	0	28	50	30,11		ENE	1	Cloudy.
	2	0	27	52	30,05		ENE	1	Cloudy.
28	7	0	27	49	29,96				Cloudy.
	2	0	31	50	29,96				Cloudy.
29	7	0	30	47	30,01		ESE	1	Cloudy.
	2	0	34	49	30,04		ESE	1	Cloudy.
30	7	0	35	48	30,18		N by W	1	Cloudy.
	2	0	35	49	30,20		NE	1	Cloudy.

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1788	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.	Deg.	Deg.	Inches.	Inch.	Points.	Str.	
Dec. 1	8	0	34	48	30,22		NE	1	Cloudy.
	2	0	36	49	30,20		ENE	1	Fine.
2	8	0	31	47	29,93		ENE	1	Fine.
	2	0	35	49	29,85		ENE	1	Fine.
3	8	0	24	46	29,68		W	1	Cloudy.
	2	0	31	48	29,61		W	1	Fair.
4	8	0	24	44	29,59		N	1	Fine.
	2	0	32	49	29,58		N	1	Fine.
5	8	0	34	45	29,68		NE	1	Cloudy.
	2	0	35	49	29,75		ENE	1	Cloudy.
6	8	0	33	45	29,84		NE	2	Cloudy.
	2	0	34	48	29,83		NE	2	Cloudy.
7	8	0	34	45	29,89		ENE	2	Cloudy.
	2	0	35	46	29,93		ENE	2	Cloudy.
8	8	0	33	44	30,00		ENE	1	Cloudy.
	2	0	38	48	30,00		E	1	Fine.
9	8	0	33	45	29,98		N	1	Fair.
	2	0	39	49	29,95		N	1	Fine.
10	8	0	34	47	29,86		N	1	Cloudy.
	2	0	40	51	29,88		ENE	1	Fine.
11	8	0	32	47	30,04				Foggy.
	2	0	35	50	30,07		WNW	1	Cloudy.
12	8	0	29	46	30,10		WNW	1	Cloudy.
	2	0	32	49	30,07		WNW	1	Fair.
13	8	0	27	45	29,96				Foggy.
	2	0	29	47	29,85		NE	1	Cloudy.
14	8	0	26,5	44	29,55		NE	1	Fair.
	2	0	30	46	29,50		NE	1	Cloudy.
15	8	0	22	42	29,61		NE	2	Cloudy.
	2	0	24	45	29,63		NE	2	Cloudy.
16	8	0	22	40	29,65		ENE	2	Fair.
	2	0	27	44	29,68		ENE	2	Fine.

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1788	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.	Deg.	Deg.	Inches.	Inch.	Points.	Str.	
Dec. 17	8	0	26	41	30,00		ENE	2	Cloudy.
	2	0	28	44	30,08		ENE	1	Snow.
18	8	0	18	40	30,14		WNW	1	Cloudy.
	2	0	28	44	30,00		WNW	1	Cloudy.
19	8	0	26	41	29,90		NNW	1	Fine.
	2	0	32	44	29,94		NNW	1	Cloudy.
20	8	0	30	42	29,98		WNW	1	Cloudy.
	2	0	33	45	29,95		WNW	1	Fine.
21	8	0	36	42	29,66		NW	2	Cloudy.
	2	0	36	44	29,68		NW	2	Cloudy.
22	8	0	29,5	42	29,97		WNW	2	Fine.
	2	0	35	46	30,03		WNW	2	Fine.
23	8	0	23	42	30,22		WNW	1	Foggy.
	2	0	25	45	30,30		WNW	1	Foggy.
24	8	0	39	43	29,96		W	2	Cloudy.
	2	0	43	47	29,96		W	1	Cloudy.
25	8	0	41	45	29,94		W	1	Fair.
	2	0	46	48	29,80		W	2	Fine.
26	8	0	31	45	29,67		NNE	2	Cloudy.
	2	0	34	48	29,90		NNE	2	Fine.
27	8	0	30	45	29 97		NNW	2	Cloudy.
	2	0	32	47	29,90		NNW	2	Cloudy.
28	8	0	23	43	30,15		NNE	1	Fine.
	2	0	26	45	30,22		NNE	1	Fine.
29	8	0	19	41	30,30		NE	1	Fine.
	2	0	26	45	30,26		NE	1	Fine.
30	8	0	18	40	30,33		E	1	Fine.
	2	0	21	44	30,31		SE	1	Fine.
31	8	0	26	39	30 03		SSW	2	Cloudy.
	2	0	30	42	29,80		SSW	2	Snow.

1788	Thermometer without.			Thermometer within.			Barometer.			Rain.
	Greateſt height.	Leaſt height.	Mean height.	Greateſt height.	Leaſt height.	Mean height.	Greateſt height.	Leaſt height.	Mean height.	
	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Inches.	Inches.	Inches.	Inches.
January	48	26	39,7	56	49	52,7	30,70	28,89	29,97	0,439
February	50	29	41,3	56	50	52,7	30,21	28,65	29,68	1,461
March	59	28	40,8	59	46	50,9	30,08	29,32	29,68	0,336
April	68	40	52,6	63	55	51,8	30,48	29,50	30,07	0,607
May	80	49	60,0	73	59	62,8	30,34	29,58	30,04	0,497
June	80	52	62,3	70	61	64,1	30,22	29,49	29,94	3,275
July	77	55	63,7	69	64	65,9	30,22	29,73	29,99	1,620
Auguſt	77	53	63,4	71	64	66,0	30,45	29,22	29,95	2,699
September	74	45	58,6	67	59	63,7	30,25	29,37	29,86	3,345
October	67	33	51,4	62	54	59,4	30,55	29,64	30,32	0,103
November	58	27	42,9	61	47	56,4	30,50	29,61	30,11	0,510
December	46	18	30,9	51	39	45,2	30,33	29,50	29,92	0,000
Whole year			50,6			57,6			29.96	14,892

END OF PART I. OF VOL. LXXIX.

PHILOSOPHICAL TRANSACTIONS.

XI. *Experiments on the Phlogiſtication of Spirit of Nitre.* *By the Rev.* Joſeph Prieſtley, *LL.D. F. R. S.*

Read March 26, 1789.

AS the colouring of ſpirit of nitre has ſome connection with the doctrine of phlogiſton, to which I propoſe to give my beſt attention, I have lately reſumed my experiments on that ſubject, and beg leave to lay the reſult of them before the Society.

In my former experiments, vol. IV. p. 2. I found that the colourleſs acid became ſmoking, or orange-coloured, and emitted orange-coloured vapours, on being expoſed to heat in long glaſs tubes, hermetically ſealed; and I then concluded, that this effect was produced by the action of *heat*, evolving, as it were, the phlogiſton previouſly contained in the acid. Afterwards, having found that it was not *heat*, but *light* only, that was capable of giving colour to ſpirit of nitre, contained in phials with ground ſtoppers, in the courſe of ſeveral days; and that in this caſe the effect was produced by the action of *light* upon the *vapour*, which gradually imparted its colour to the liquor on which it was incumbent (ſee Vol. V. p. 342.), I was led to ſuſpect, that as the glaſs tubes, in which I had formerly expoſed this acid to the action of heat, were only held near to a fire, in the day-light, or candle-light, it might have been this *light*, which, in theſe circumſtances, had, at leaſt in part, contributed to produce the effect.

In order to aſcertain whether the light had had any influence in this caſe, I now put the colourleſs ſpirit of nitre into long glaſs tubes, like thoſe which I had uſed before, and alſo ſealed them hermetically, as I had done the others; but, inſtead of expoſing them to heat in the open air, from which light could not be excluded, I now ſhut them up in gun barrels, cloſed with metal ſcrews, ſo that it was impoſſible for any particle of light to have acceſs to them; and I then placed one end of the barrels ſo near to a fire as was ſufficient to make the liquor contained in the tube to boil, which I could eaſily diſtinguiſh by the ſound which it yielded. The conſequence was, that in a ſhort time the acid became as highly coloured as ever it had been when expoſed to heat without the gun barrel. It was evident, therefore, that it had been mere *heat*, and not *light*, which had been the means of giving this colour to the acid, and which has been uſually termed *phlogiſticating* it.

When I made the former experiments, I had no ſuſpicion that the *air* contained in the tube had any concern in the reſult of them; and, in thoſe which I made in the phials in a moderate heat, I found that the acid received its colour when the beſt *vacuum* that I could make with an air pump was over it.

My friend Mr. KIRWAN, however, having always ſuſpected, that the *air* was a principal agent in the buſineſs, I at this time gave particular attention to this circumſtance; ſuppoſing that, if any part of the common air had been imbibed, it muſt have been the *phlogiſticated*, and that it was the phlogiſton from this kind of air which had phlogiſticated the acid. The real reſult, however, was not ſo much in favour of this ſuppoſition as I had expected; for the principal effect of the proceſs was the emiſſion of dephlogiſticated air, ſo that the acid ſeems to become what we call phlogiſticated, by parting with this ingredient in its compoſition.

I put a ſmall quantity of the colourleſs acid into a long glaſs tube, which beſides the acid would have contained 1.23 ounce meaſures of common air, but that the vapour of the acid excluded about one-twentieth of the quantity. Having ſealed the tube hermetically, I ſhut it up in a gun barrel, in the manner mentioned above, and expoſed it to a boiling heat for ſeveral hours, and then opening it under water there came out of it 2.03 ounce meaſures of air, very turbid and white; and when it was examined, it appeared to be of the ſtandard of 1.02, with two equal meaſures of nitrous air; when with one meaſure of the ſame nitrous air the ſtandard of the common air was 1.07. The quantity of phlogiſticated air abſorbed in this experiment I aſcertained by the following computation.

As one meaſure of common air, and an equal quantity of nitrous air were reduced to 1.07 m. it is evident, that 0.93 m.

had diſappeared; but as this was effected by the nitrous air uniting with all the dephlogiſticated air contained in the common maſs, and as they unite in the proportion of one meaſure of dephlogiſticated air to two meaſures of nitrous air, one-third of the 0.93 m. *viz.* 0.31 m. will be the quantity of dephlogiſticated air that was contained in the one meaſure of common air on which the experiment was made, the remainder, *viz.* 0.69, having been phlogiſticated air. The common air contained in the tube would have been 1.23 oz. m.; but deducting from it one-twentieth in the whole, it will only be 1.17 oz. m. I then ſay, if one meaſure of this air contains 0.69 m. of phlogiſticated air, 1.17 oz. m. will contain 0.8073 oz. m. of phlogiſticated air. This, therefore, was the quantity of phlogiſticated air which had been expoſed to the action of the acid of nitre in the tube.

In order to find how much of the ſame kind of air was contained in the tube *after* the proceſs, I examined the reſult above mentioned in the following manner. Since two meaſures of nitrous air, and one of this reſiduum, were reduced to 1.02 m. it is evident, that 1.98 m. had diſappeared, and conſequently one-third of this quantity, *viz.* 0.66 m. had been dephlogiſticated air, and that the remainder of the meaſure, *viz.* 0.34, had been the proportion of phlogiſticated air in one meaſure of this reſiduum. If then one meaſure of this reſiduum contains 0.34 m. of phlogiſticated air, 2.03 oz. m. will contain 0.6902 oz. m. which is leſs than 0.8073 oz. m. the quantity contained in it before the proceſs; ſo that a part of the phlogiſticated air had been either abſorbed or decompoſed, its phlogiſton having been imbibed by the acid at the ſame time that it had emitted the dephlogiſticated air.

In another proceſs, of the ſame kind, the glaſs tube contained 0.92 oz. m. of common air, and the air that came out of it after the proceſs was one ounce meaſure, of the ſtandard of 1.6 with two meaſures of nitrous air, and computing as I did before, the phlogiſticated air in the tube before the proceſs was 0.6072 oz. m., and after the proceſs 0.54 oz. m.

In theſe computations it is ſuppoſed, that the air emitted by the acid was perfectly pure, ſo that all the phlogiſticated air that is found after the proceſs is ſuppoſed to have been contained in the common air confined in the tube before it was commenced. But I found, that the air emitted by the acid is by no means perfectly pure, ſo that much of the impurity muſt be aſcribed to this circumſtance.

In order to exclude all air from the contact of the acid, I made a quantity of it to boil in the tube, and when the vapour had expelled all the air, I ſealed it hermetically, in the manner in which water hammers are made; and then expoſing it to heat, found that it acquired as high a colour as when air had been confined along with it; ſo that it is evident, that *air* is not neceſſary to this effect. When the tube was opened under water, a quantity of dephlogiſticated air ruſhed out, exceedingly white as before; but when I examined it, I found it to be of the ſtandard of only 0.66. When this impurity is conſidered, it will appear, that when much air is yielded in this proceſs, ſome phlogiſticated air may have been imbibed, though, computing in the manner above mentioned, the phlogiſticated air after the proceſs ſhould be in greater quantity than was contained in the tube before it, as was the caſe in the following experiment.

In a glaſs tube which, beſides the acid, contained 1.13 oz. m. of common air, I expoſed colourleſs ſpirit of nitre to heat till it became of a deep orange colour; and when it was opened under water, there came out of it 2.83 oz. m. of air exceedingly turbid, of the ſtandard of 0.66, with two equal quantities of nitrous air, when that of the common air, with one equal quantity of nitrous air, was 1.07. Computing in the manner above mentioned, there was in the tube before the proceſs 0.7477 oz. m. of phlogiſticated air, and after the proceſs 0.8792 oz. m. But the dephlogiſticated air, amounting to 1.7 oz. m. being of the ſtandard of 0.66, will be found to contain 0.374 oz. m. of phlogiſticated air, which being deducted from 0.8792, there will remain only 0.5052 oz. m. which is conſiderably leſs than 0.7477 oz. m.

That the nitrous acid can become coloured, without imbibing any thing from phlogiſticated air, is evident not only from its becoming ſo when heated *in vacuo*, as deſcribed above, but alſo, when it was in contact with any other kind of air, as free from phlogiſticated air as I could make it. But from the manner in which theſe experiments were neceſſarily made, it was impoſſible intirely to exclude phlogiſticated air, either as part of the atmoſpheric air, or as contained in the impurities of the air that I made uſe of; for I firſt filled the tube with ſpirit of nitre, then plunging the orifice of it in a veſſel of the ſame, I introduced a quantity of the air which I wiſhed to expoſe to it. After this, putting my finger upon the orifice, I turned it upſide down, and applying to it the cloſed end of a glaſs tube, of about the ſame diameter, I ſealed it hermetically with a blowpipe as expeditiouſly as I could. This is a neceſſary imperfection in the experiment; but I know not how to remedy it, if any of the acid is to be left in the tube. However, the phlogiſticated air introduced in this manner from the atmoſphere muſt have borne a very ſmall proportion to the air in the tube; and ſome objection will always remain to the experiment from the impurity of the air made uſe of.

Having repeatedly obſerved, that the acid became coloured in conſequence of being expoſed to heat in contact with any kind of air whatever, I expoſed at the ſame time, and in the ſame circumſtances, three equal quantities of the ſame colourleſs ſpirit of nitre, in three nearly equal tubes, one containing dephlogiſticated, another phlogiſticated, and a third inflammable air; that, if there ſhould be any difference in the colouring of the acid in theſe caſes, it might be the more eaſily perceived. But though I gave all the attention that I could, I did not perceive that there was any difference, except what aroſe from ſome of the tubes being placed a little nearer the fire than the reſt; and, by changing their places, the colour was at length the very ſame in them all.

As in theſe three caſes I examined the air before and after the proceſs, in the manner above mentioned, I ſhall juſt recite the particulars.

Of the dephlogiſticated air the tube contained before the proceſs 1.46 oz. m. of the ſtandard of 0.67, and after the proceſs it contained 1.76 oz. m. of the ſtandard of 0.77; a difference owing in part to the mixture of common air, which could not be excluded in the ſealing of the tube, and in part to the air emitted from the acid not being pure.

Of the phlogiſticated air, the tube contained 1.3 oz. m. and after the proceſs 1.95 oz. m. of the ſtandard of 1.38.

Of the inflammable air, the tube contained before the proceſs 1.52 oz. m. and after the proceſs 1.9 oz. m. of the ſtandard of 1.8. They were all meaſured by a mixture of two equal quantities of nitrous air.

If theſe reſults be examined as that of the firſt experiment, with common air, it will be found that, in all theſe proceſſes, there was leſs phlogiſticated air, or inflammable air, after the proceſs than before; and this reſult being thus uniform, I cannot help concluding, that this kind of air is in part decompoſed, and purified by this means; ſo that by this emiſſion of dephlogiſticated air which the heat expels from the acid, ſomething, and probably phlogiſton, is at the ſame time imbibed from it; which proves that phlogiſticated air is no ſimple ſubſtance, but a compound, and that phlogiſton is one conſtituent part of it; for this acid acquires the ſame colour, and all the ſame properties, by adding to it any thing that is ſuppoſed to contain phlogiſton.

As the ſpirit of nitre can be rendered ſmoking, or phlogiſticated, by the mere expulſion of dephlogiſticated air, it is evident, that it contains two principles in cloſe affinity with each other, and that nothing is neceſſary to render either of them conſpicuous beſides the abſence of the other.

It is alſo natural to ſuppoſe, that, for the ſame reaſon that the *dephlogiſticating* principle (as it may be called) is expelled, the *phlogiſticating* principle ſhould enter; ſo that the purification of the air in contact with the acid may be a neceſſary conſequence of the expulſion of the pure air contained in it, the whole tending, as it were, to an equilibrium in this reſpect. It is therefore by no means difficult to conceive, that phlogiſton ſhould be extracted from the contiguous air at the ſame time that the dephlogiſticated air *not pure* (that is, containing a mixture of phlogiſticated air) is driven out of it; for the acid always containing phlogiſtion, whatever air is contained in it, and expelled from it, may neceſſarily contain phlogiſton or phlogiſticated air; but the purer air may be emitted, and the leſs pure air be imbibed, till the whole come to be of the ſame quality. It may, however, perhaps follow from the emiſſion of impure dephlogiſticated air, and the imbibing of phlogiſticated air at the ſame time, that the former does not conſiſt of dephlogiſticated and phlogiſticated air looſely mixed, but of ſome intimate union of dephlogiſticated air with phlogiſton, though they may be ſeparated by a mixture of nitrous air, and other proceſſes, in the very ſame manner as dephlogiſticated air may be ſeparated from a looſe mixture of phlogiſticated air.

It is evident from theſe experiments, that a red heat is not neceſſary to the converſion of nitrous acid into pure air, though this proceſs, as appeared by my former experiments, produces this effect moſt quickly and effectually.

I cannot help conſidering the experiments above recited to be favourable to the doctrine of the phlogiſton, and unfavourable to that of the decompoſition of water, though not deciſively ſo; for ſince the red vapour of ſpirit of nitre unqueſtionably contains the ſame principle that has been termed phlogiſton, or the principal element in the conſtitution of inflammable air, and according to the antiphlogiſtians this is one conſtituent part of water, they muſt ſuppoſe, that the water in this acid is decompoſed by a much more moderate heat than in moſt other caſes. In general, I believe, they have thought a red heat to be neceſſary for this purpoſe. It is evident, that the converſion of water into ſteam by boiling, or by any heat that can be given to it under the ſtrongeſt preſſure, has no tendency whatever to decompoſe it. But if the mere boiling of water in nitrous acid could produce this effect, I do not ſee why the ſame ſhould not be the caſe when water alone is boiled.

I think it will alſo be more difficult to explain the purification of the incumbent atmoſpherical air on the antiphlogiſtic

than

than on the phlogiſtic hypotheſis, whatever be the conſtitution of phlogiſticated air.

As, in the experiments above mentioned, *heat* without *light* gives colour to the nitrous acid, and the reflection or refraction of light is always attended with heat, it may perhaps be *heat* univerſally that is the means of imparting this colour, though the mode of its operation be at preſent unknown. And in theſe experiments, as well as the former, it is the *vapour* that firſt receives the colour, and imparts it to the liquid when it is ſufficiently cold to receive it.

The ruſhing out of a quantity of turbid white air from a tranſparent tube, quite cold, is a ſtriking phænomenon in theſe experiments. It may be worth while to examine of what it is that this remarkable cloudineſs of the air conſiſts. There is the ſame appearance, as I have more than once obſerved, in the rapid production of any kind of air, which is perfectly tranſparent as it paſſes along the glaſs tube through which it is tranſmitted, till it comes into contact with the water in which it is received.

P. S. Not to multiply my communications on the ſubject of *phlogiſton* unneceſſarily, I would beg leave to obſerve, at the cloſe of this article (in reply to what has been objected to my former experiments, as being liable to exception from the phlogiſticated air which could not be excluded from the dephlogiſticated air when it was decompoſed by means of inflammable air) that I have found the proceſs I made uſe of to have no tendency whatever to decompoſe phlogiſticated air. Indeed, nothing that we have hitherto known concerning this kind of air could make it probable, that mere *heat*, in contact with dephlogiſticated or inflammable air, *could* have this effect. And it is of no conſequence whatever to ſay, that any particular ſubſtance, imagined to be decompoſed, is *preſent* in a proceſs, unleſs it can be ſhewn that, in that proceſs, there are agents capable of decompoſing it. If mere *heat* (which is all that my proceſs requires) would decompoſe phlogiſticated air, and reduce it to nitrous acid, the tranſmiſſion of common air (which conſiſts of dephlogiſticated and phlogiſticated air) through a red hot tube would have this effect, which it is well known not to have.

But what I have aſſerted above is a concluſion which I have drawn from comparing the decompoſition of dephlogiſticated air by the two proceſſes with nitrous and inflammable air. That nitrous air, when mixed with dephlogiſticated air, has no tendency to produce phlogiſticated air, is evident from the almoſt total evaneſcence of both of them, when they are very pure, and mixed in due proportions; and that nitrous air has no effect on phlogiſticated air is well known. If then the firing of dephlogiſticated and inflammable air had a tendency to decompoſe any portion of phlogiſticated air, which ſhould happen to be mixed with them, leſs would remain after the firing of inflammable and impure dephlogiſticated air than after mixing it with nitrous air; for as the impurities of dephlogiſticated air conſiſt of phlogiſticated air, thoſe would diſappear in a greater proportion in the former proceſs than in the latter. But by many careful trials I find, that I can reduce any kind of dephlogiſticated air no farther by a mixture of inflammable air than I can by nitrous air. When the proportions are well managed, the diminution is as nearly as poſſible the ſame in both the caſes.

I muſt obſerve, however, that it requires more nitrous air than inflammable air (from iron by ſteam) to produce this effect in the proportion of about 10 to 9; ſo that nitrous air does not contain quite ſo much phlogiſton as an equal bulk of inflammable air, as I had before thought to be the caſe.

In this Paper it will be obſerved, that I make the diminution of common air by nitrous air to be conſiderably leſs than I have uſually done before. This has been the conſequence of giving the two kinds of air a little agitation at the inſtant of mixing, which will generally make the diminution leſs by two tenths of a meaſure. But I have found, that when theſe mixtures of air, with and without agitation, have been kept ſome time, they approach to an equality of bulk.

At the ſame time I have obſerved, what I think not a little extraordinary, that agitation prevents the greateſt diminution of dephlogiſticated and nitrous air. I have found it to be 2.5 without agitation, and 6. with it.

The leſs diminution of the mixture of nitrous and common air is probably owing to the preſence of ſo much phlogiſticated air, which impedes the meeting of the nitrous air with the dephlogiſticated air in the mixture; becauſe I find the ſame to be the caſe when I mix the ſame proportion of inflammable air with dephlogiſticated air; and when dephlogiſticated air is agitated with nitrous air, the *water* may impede their union, as the phlogiſticated air did before.

There is, therefore, no ſource of the *nitrous acid* which I find on the decompoſition of dephlogiſticated and inflammable air, beſides the union of thoſe two kinds of air, which therefore do not make *mere water*, as the antiphlogiſtians ſuppoſe.

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XII. *Obſervations on a Comet. In a Letter from* William Herſchel, *LL.D. F. R. S. to Sir* Joſeph Banks, *Bart. P. R. S.*

Read April 2, 1789.

SIR, Slough, March 3, 1789.

THE laſt time I was in town, you expreſſed a wiſh to ſee my obſervations on the comet which my ſiſter, CAROLINE HERSCHEL, diſcovered in the evening of the 21ſt of laſt December, not far from β Lyræ.

As ſhe immediately acquainted the Rev. Dr. MASKELYNE, and ſeveral other gentlemen, with her diſcovery, the comet was obſerved by many of them. The Aſtronomer Royal, in particular, having, I find, obtained a very good ſet of valuable obſervations on its path, it will be ſufficient if I communicate only thoſe particulars which relate to its firſt appearance, and a few other circumſtances that may perhaps deſerve to be noticed.

December 21, 1788, about 8 o'clock, I viewed the comet which my ſiſter had a little while before pointed out to me with her ſmall Newtonian *ſweeper*. In my inſtrument, which was a ten-feet reflector, it had the appearance of a conſiderably bright nebula; of an irregular, round form; very gradually brighter in the middle; and about five or ſix minutes in diameter. The ſituation was low, and not very proper for inſtruments with high powers.

December 22, about half after five o'clock in the morning, I viewed it again, and perceived that it had moved apparently in a direction towards δ Lyræ, or thereabout. I had been engaged all night with the twenty-feet inſtrument, ſo that there had been no leiſure to prepare my apparatus for taking the place of the comet; but in the evening of the ſame day, I took its ſituation three times, as follows:

	h.	′	″	
Dec. 22. at	23	42	19	ſidereal time, the comet paſſed the wire,
at	23	49	24	β Lyræ paſſed the ſame,
Difference		7	5	very accurate.
at	23	52	52	the comet paſſed,
at	23	59	58	β Lyræ paſſed,
Difference		7	6	accurate.
at	0	6	35	the comet paſſed,
at	0	13	40	β Lyræ paſſed,
Difference		7	5	very accurate.

I found in every obſervation the ſmall ſtar which accompanies β Lyræ *, exactly in the parallel of the comet.

Theſe tranſits were taken with a ten-feet reflector; and the difference in right aſcenſion, I ſhould ſuppoſe, may be depended upon to within a ſecond of time. The determination

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* For this ſmall ſtar ſee my Catalogue of Double Stars, in the Philoſophical Tranſactions for the year 1782, Part I. Claſs V. Star 3. where its diſtance and poſition are given, and conſequently its parallel may be found.

alſo of the parallel can hardly err ſo much as fifteen ſeconds of a degree.

This, and ſeveral evenings afterwards, I viewed the comet again with ſuch powers as its diluted light would permit, but could not perceive any ſort of nucleus, which, had it been a ſingle ſecond in diameter, I think, could not well have eſcaped me. This circumſtance ſeems to be of ſome conſequence to thoſe who turn their thoughts on the inveſtigation of the nature of comets; eſpecially as I have alſo formerly made the ſame remark on one of the comets diſcovered by M. MECHAIN in 1787, a former one of my ſiſter's in 1786, and one of Mr. PIGOTT's in 1783; in neither of which any defined, ſolid nucleus could be perceived.

I have the honour to remain, &c.

WILLIAM HERSCHEL.

continued.

Dates.	Snow-drop flower.	T f	Aſh leaf.	Ringdoves coo.	7	Turnip in flower.	Lime leaf.	Maple leaf.	Wood Anemone flower.
1756	Jan. 8.	Ja		Feb. 22.		1755, Dec. 28. and March 4.	May 7.		
1757	Feb. 6.	Fe	May 1.	Mar. 10.	I	April 11.			
1758	Jan. 9.	F	May 8.	March 3.	5.	April 14.			
1759	Jan. 9.	Ja	April 23.	Feb. 11.		Feb. 27	April 18.	April 18.	
1760	Jan. 24.	Fe	April 19.		N N	April 13.	April 19.	April 13.	
1761		Ja	May 3.	March 20.	N.	Jan. 15.	April 4.	April 17.	March 29.
1762	Jan. 12.	Ja	April 25.			April 19.	April 16.	April 21.	
1763		Lo Fel	May 7.			April 17.	April 15.		
1764	Jan. 6.	Ja	Kent, May 5.	Feb. 22.	N	Middleſex, March 18.			
1765	Jan. 5.	Fel	Herts. May 1.	Feb. 25.	N		Middleſex, April 10.		March 24.
1766		Fel	Surrey, May 2.	March 3.	N		Surrey, April 12.		
1767	Jan. 28	Fel					London, March 22.		
1768	Jan. 28.		April 27.		M F	March 28.	April 20.		
1769	Jan. 13.		Eſſex, May 4.				London, April 16.		
1770	Jan. 21.	Ja	May 12.		F.	April 23.	May 2.	May 7.	

I

ing continued.

Dates.	Snow-drop flower.	Th fa	beam f.	Aſh leaf.	Ringdove coo.	ellow utterfly ppears.	Turnip in flower.	Lime leaf.	Maple leaf.	Wood Anemone flower.
1771	Jan. 27.	Fe	7.	May 16.	Feb. 26	eb. 18.	May 3.	May 7.	May 7.	April 11.
1772	Jan. 14.	Fe	3.	May 26.	March	arch 25.	April 17.	Middleſex, April 18.	May 5.	
1773	Jan. 11.	Ja	10.	May 20.	March 3	arch 3.	March 24.	April 21.	April 19.	April 8.
1774	Jan. 28	B Fe	16.	April 27.		Herts. arch 4	April 6.	April 8.	April 24.	March 26.
1775	Jan. 14.	Fe	dgeſh. 31.	May 2.	Feb. 15	eb. 26.	March 15	Cambridgeſh. March 31.	Herts. April 7.	March 9.
1776	1775 Dec. 30.	Fe	2.	April 23.	Feb. 14	arch 22.	April 7.	April 12.	April 6.	April 3.
1777	Jan. 17.	Fe	11.	May 4.	Feb. 28	eb. 27.	April 7.	April 10.	April 22.	
1778	Jan. 26.	F	12.	April 25.	Feb. 9	arch 18.	April 14.		April 13.	April 5.
1779	1778 Dec. 24.	F	24.	April 2.	Feb. 25	eb. 18.	Feb. 28.	April 1.	March 31.	March 15.
1780	Feb. 9.	Fe	29.	May 1.	March	arch 8.	April 30.	April 2.	April 22.	March 29.
1781	Jan. 29.	F	3.	April 19.	Feb. 9	arch 9.	April 7.	April 6.	April 15.	March 26.
1782	Jan. 4.	F	22.	May 16.	Feb. 2	April 7.	April 10.	May 5.	April 27.	April 12.
1783	Jan. 26.	Ja	14.	April 20.	Feb. 1	arch 18	March 23.	April 2.	April 18.	April 1.
1784	Feb. 8.	Ma	27.	May 12.	March	pril 17.	May 14.	May 4.	April 27.	April 22.
1785	Jan. 24.	Ja	19.	May 2.	March 1	arch 19.	April 27.	April 22.	April 24	April 16.
1786	Jan. 22.	F	18.	May 2.	March 1	arch 12.	April 25.	April 20.	April 21.	April 11.
1787	Jan. 30.	F	18.	April 29.	March	eb. 18.	March 22.	March 29.	April 26.	March 16.
1788	Jan. 12.	Fe	4.	April 20.	March 1	arch 28.	April 13	April 11.	April 12.	March 30.

XIV. *An Account of a Monſter of the human Species, in two Letters; one from Baron* Reichel *to Sir* Joſeph Banks, *Bart. and the other from Mr.* James Anderſon *to Baron* Reichel. *Communicated by Sir* Joſeph Banks, *Bart. P. R. S.*

Read April 30, 1789.

TO SIR JOSEPH BANKS, BART.

SIR, Fort St. George, Feb. 28, 1788.

I HAVE the pleaſure to tranſmit to you the portrait of a Gentoo boy, an aſtoniſhing living ſubject, who being ſent to me by a friend of mine reſiding in the environs of the native place of the boy, I made two drawings repreſenting the alternate attitudes in which he can place half the body of his little brother, who adheres to his breaſt. See Tab. II. PERUNTALOO is a handſome well-made lad, poſſeſſing every due faculty of mind and body, rather more ſagacious, and with a ſuperior ſhare of underſtanding, than young men in general of his age. In addition to the incloſed anatomical deſcription of the boy by Mr. ANDERSON, you will obſerve in the drawings two circular dotted lines, about the lower part of the loins of the ſemi-monſter. During the ſeveral ſittings I had of PERUNTALOO, I obſerved an internal motion about theſe parts rather more conſpicuous than any other of the body; and upon queſtioning the youth, he ſhewed me, that by retaining his breath, he could force a current of air into them, ſo as to ſwell the parts like two blown-up bladders, with a rumbling noiſe at the time of action. Whether there is a connection with the lungs of PERUNTALOO is a queſtion a queſtion I cannot venture to determine; Mr. ANDERSON, however, thinks it well worth my mentioning this obſervation. The erection of the little penis in the ſemi-monſter, and the command PERUNTALOO has of diſcharging the urine through it, are perfectly aſcertained.

Such as this ſubject is, if with any merit, you may depend upon the correctneſs of the drawings.

I am, &c.

T. REICHEL.

TO BARON REICHEL.

SIR, Fort St. George, Feb. 25, 1788.

AS you mean to ſend the elegant drawing of PERUNTALOO to Sir JOSEPH BANKS, you may acquaint him from me, that the little brother is ſuſpended by the os pubis; an elongation of the ſword-like cartilage of PERUNTALOO having anaſtomoſed with that bone at the ſymphyſis.

The lower orifice of the ſtomach ſeems to lie in the ſac or cylindrical cavity between the two brothers on the right-ſide, and what may be reckoned the right hypochondre of the little one, as that part is tumid and full after eating.

The alimentary canal muſt be common to both, as the anus of the little one is imperforate.

There is a bladder of urine diſtinctly perceived, which occupies the left ſide of the ſac, or left hypochondre of the monſter.

Beſides which, there remain only the ſacrum, oſſa innominata, and lower extremities perfect.

Peruntaloo ſays he has as complete a ſenſe of feeling with every part of the body of his little brother as of his own proper body, and this may account for the erections you ſaw, and making water diſtinctly; but this volition does not extend to the legs or feet, which are cold in compariſon with the reſt.

I am, &c.

JAMES ANDERSON.

EXPLANATION OF THE PLATE.

Peruntaloo, ſon of **Chindrahpah-Nayandoo**, of the Gentoo Caſt. He was born at Popelpahdoo, 70 miles weſt from Muſilipatnam. He is 13 years of age, and meaſures 4 feet 6½ inches in height.

Fig. 1. Natural poſition.

Fig. 2. Reverſed poſition.

* * *

XV. *A ſupplementary Letter on the Identity of the Species of the* Dog, Wolf, *and* Jackal; *from* John Hunter, *Eſq. F. R. S. addreſſed to Sir* Joſeph Banks, *Bart. P. R. S.*

Read April 30, 1789.

SIR,

IN the year 1787 I had the honour of preſenting to this learned Society, a Paper to prove the Wolf, the Jackal, and the Dog to be of the ſame ſpecies. But as the complete proof of the Wolf being a Dog, which conſiſted in the half-bred puppy breeding again, had not been under my own inſpection, although ſufficiently well authenticated, I ſaved a female of one of the half-bred puppies, mentioned in that Paper, in hopes of being myſelf a witneſs of the fact; but when the period of impregnation arrived, we unluckily miſſed that opportunity. However, another half-bred puppy has had young, which is equally ſatisfactory to me as if my own had bred. JOHN SYMMONS, Eſq. of Milbank, has had a female Wolf in his poſſeſſion for ſome time, who was lined by a Dog, and brought forth ſeveral puppies, which I had the honour of ſeeing with you. This was a very ſhort time after the brood had been produced by Mr. GOUGH's Wolf, the ſubject of my former Paper, therefore the puppies were nearly of an age with mine. Theſe puppies Mr. SYMMONS has reared; only one of them was a female, and ſhe had much more of the mother or Wolf in her than any of

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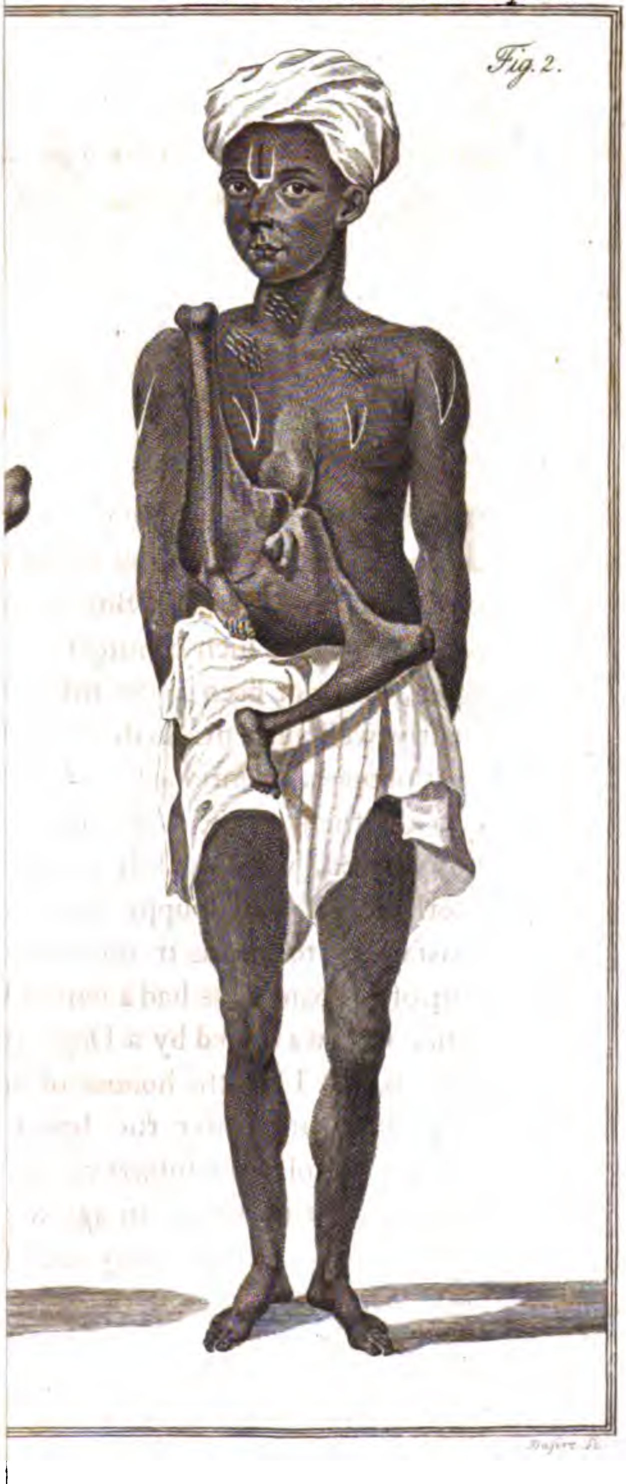
Fig. 2.



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the reſt of the ſame litter. I communicated my wiſh to Mr. SYMMONS, that either his puppy or mine ſhould prove the fact to our own knowledge; which he immediately, with great readineſs, acceded to. On the 16th, 17th, and 18th of December, 1788, this bitch was lined by a Dog, and on the 18th of February ſhe brought eight puppies, all of which ſhe now rears. If we reckon from the 16th of December, ſhe went ſixty-four days; but if we reckon from the 17th, the mean time, then it is ſixty-three days, the uſual time for a bitch to go with pup. Theſe puppies are the ſecond remove from the Wolf and Dog, ſimilar to that given by my Lord CLANBRASSIL to the Earl of PEMBROKE, which bred again. (See Philoſophical Tranſactions, Vol. LXXVII. p. 255.) It would have proved the ſame fact if ſhe had been lined by either a Wolf, a Dog, or one of the males of her own litter.

I may juſt remark here, that the Wolf ſeems to have only one time in the year for impregnation natural to her, and that is in the month of December; for every time Mr. GOUGH's Wolf has been in heat was in this month, and it proves to be the ſame month in which Mr. SYMMONS's Wolf was in heat; for his half-bred Wolf is nearly of the ſame age with mine, and the time ſhe was in heat was alſo the ſame with that of her own mother, and the preſent brood correſponds in time with the brood of Mr. GOUGH's Wolf.

I am, &c.

JOHN HUNTER.

XVI. *Abſtract of a Regiſter of the Barometer, Thermometer, and Rain, at* Lyndon *in* Rutland; *by* Thomas Barker, *Eſq. Alſo of the Rain in* Hampſhire *and* Surrey. *Communicated by* Thomas White, *Eſq. F.R.S.*

Read April 30, 1789.

1788.

		Barometer.			Thermometer.						Rain.			
					In the Houſe.			Abroad.			Lyndon	Surrey. S. Lambeth.	Hampſhire. Selbourn.	Fyfield.
		Higheſt	Loweſt.	Mean.	High.	Low.	Mean	High.	Low.	Mean				
		Inches.	Inches.	Inches.	°	°	°	°	°	°	Inch.	Inch.	Inch.	Inch.
Jan.	Morn.	30,13	28,37	29,50	44	34	40	45	28½	36	0,970	0,68	1,60	1,10
	Aftern.				44	35	40½	49	30	41				
Feb.	Morn	29,77	28,25	29,14	45	35	40½	44	27½	36	2,667	2,09	3,37	2, 6
	Aftern.				45	37	41	48	30	42				
Mar.	Morn.	29,65	28,84	29,23	51	34	40	50	22	35	1,072	0,64	1,31	1,36
	Aftern.				52½	35½	41	63	31	43				
Apr.	Morn.	30,02	28,94	29,59	56	42½	50	54	35	45½	0,588	0,47	0,61	0,50
	Aftern.				60	43½	51	68½	40	56				
May	Morn.	29,92	29,19	29,60	68½	51	58	64	43½	52½	1,517	0,81	0,76	0,28
	Aftern.				72	53	60	82	51	66				
June	Morn.	29,85	29,10	29,52	65½	56	60	64½	50	56	0,608	1,94	1,27	1,36
	Aftern.				69	57½	61	82½	58	67				
July	Morn.	29,78	29,21	29,52	67½	58	62	70½	51	59	1,795	1,84	3,58	1,81
	Aftern.				70	59½	63½	83	58	72				
Aug.	Morn.	30,01	28,88	29,49	68	57½	61½	64	54	56	2,780	4,30	3,22	3,40
	Aftern.				70½	59	63	77	62	68				
Sept.	Morn.	29,80	29,00	29,40	66	52½	58½	61½	42	52	2,430	3,81	5,71	3,78
	Aftern.				66½	53	59	75½	50	63				
Oct.	Morn.	30,15	29,15	29,68	59	46	52	57	32	46	1,412	0,08	0, 0	0,03
	Aftern.				60	47	53	66	45	54½				
Nov.	Morn.	30,11	29,06	29,62	53	37	45	51½	25½	39	0,453	0,62	0,86	0,74
	Aftern.				53	37½	46	58	31	45				
Dec.	Morn.	29,85	29,12	29,47	39	27	34	40½	15	27	0,890	0,00	0,21	0,42
	Aftern.				49½	28½	34½	44½	22½	31½				
										Inches	17,182	17,28	22,50	16,84

THE year began open and mild, at firſt ſhowery, afterward drier and ſtormy. The chief part of February was wet, but more ſo for frequency than quantity. After a few miſty days there came in March above a fortnight's ſharp froſt, the longeſt this winter, and with ſevere eaſt winds cut things more than all the winter before, which was in general an open one. The laſt twelve or thirteen days of March the ſpring ſet in pleaſant, and continued forwarding all April, and proved a very dry ſpring. There were at times this year fits of exceeding hot weather, the end of April, the fourth week in May, the third in June, and ſecond in July; but ſo much windy weather, with hot ſun and cold winds, that bees which were forward the beginning of May, and ſome few ſwarms ſo early, ſeemed backwarder again at the end of the month. The graſs was every where ſhort, and began to burn; but a fine rain at the end of May ſtrengthened the grain very much, and made the graſs grow in ſome degree; but it ſoon began to burn again in a dry June, with almoſt conſtant north and north-eaſt winds, ſo that the paſture was ſhort, and very little hay.

The end of June, and two-thirds of July, were very frequent ſhowers and wind. There were in ſome parts of England very heavy thunder-ſtorms, and more rain than they wiſhed for in hay-time. The ſhowers were light here; they made the grain ear well, peaſe and beans ſet thick, and brought the turnips paſt the fly. The graſs alſo grew in ſome degree, but burnt again before July was out, and more in Auguſt, of which the firſt ten days were dry; but the ſhowery latter part made the graſs grow conſiderably, which was much wanted, and did not much hinder the harveſt, which was in general well got, and was good. The autumn was very fine, and ſo much rain in one month, eſpecially the third week in September, that there was more graſs after that than there had been any part of the ſummer before, though not ſuch quantities as there ſometimes is; for, take the year throughout, I think I never knew leſs. But that was not the caſe in all parts of England: I believe it was in general a dry ſummer every where; but in ſome places there was a great deal of graſs at times. So great a fruit year of moſt ſorts, garden, orchard, and wild, I think, I hardly ever knew. After the firſt week in October it was dry again, and ſo fine, mild, and clear of froſts, that the naſturtiums were not cut off till after the middle of November; and the ground and roads continued dry till the ſnow at Chriſtmas, and there was in many places great want of water ſo late in the year. Moſt part of the laſt week in November, and the firſt third part of December, was a gentle froſt; but then it ſet in very ſevere, and, except an imperfect thaw the 24th and 25th, has been an uncommonly cold and hard froſt, freezing over many of the rivers, with a conſiderable ſnow at times, chiefly the 26th and 27th, and continued to the end of the year, and beyond it.

Account of a ſinking-in of the ground.

In a wet ſeaſon, about Chriſtmas 1787, a piece of apparently ſound ground on the north ſide of a moderate hill, a mile and half ſouth-weſt from Ketton in Rutland, ſunk down into the earth, leaving a great hollow. The ground was ſmooth before, and a waggon had lately gone over the place. There was nobody by when it fell in; but a labourer going home from his work was the firſt perſon who found it.

It was ſome time after the accident before I heard of it, and it was in ſpring time that I went to ſee it. I then found it to be an oval hole, five yards over one way, and four another, and about four yards deep in the middle; but ſome of the earth having lodged againſt the ſides of the pit, it was not ſo deep there; yet the oval muſt upon the whole have ſunk down about three yards, and gone directly downward, for the ſides of the pit are left perpendicular. I found a little water at the bottom of the pit, and was told there had been a great deal more at firſt. The bottom half of the pit is a blue clay, and from a foot to a yard thick at the top is a ſtiff earth mixed with ſtones. There were plain ſigns that a drain from the ground above had in wet times run down near where the pit now is; ſome of it probably ran into and under the ground, and had, in a courſe of time, undermined it; and that ſeems to have been the reaſon that the pit ſunk in as it has done.

A man of Ketton, who has freeſtone pits in the ſame lordſhip, but on the oppoſite ſide of the town, ſays, he ſometimes meets with beds of clay in his pits, which are undermined, and have hollows in them. And to the northward of theſe ſtone pits there are many hollows, which they call the Swallow-pits; becauſe, being hollow underneath, no water will lie in them, but runs through holes into the ground. Theſe ſwallow-pits I know, and they ſeem to be clay at top; and he ſays, they do not appear to have been ever dug by men, but that the ſurface of the ground has ſunk down into the hollow there was beneath it.

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XVII. *On the Method of correſpondent Values*, &c. *By* Edward Waring, *M. D. F. R. S. and* Lucaſian *Profeſſor of the Mathematics at* Cambridge.

Read May 28, 1789.

I.

1. IN the year 1762 I publiſhed a method of finding when two roots of a given equation $x^n - px^{n-1} + qx^{n-2} - rx^{n-3} +$ &c. $= 0$ are equal, by finding the common diviſors of the two quantities $a^n - pa^{n-1} + qa^{n-2} -$ &c., and $na^{n-1} - \overline{n-1}pa^{n-2} + \overline{n-2}qa^{n-3} -$ &c., and obſerved if they admitted only one ſimple diviſor $(a - A)$, then two roots were only equal; if a quadratic $(a^2 - Aa + B)$, then two roots of the equation became twice equal; if a cubic $(a^3 - Aa^2 + Ba - C)$, then two roots became thrice equal; and ſo on: or, to expreſs in more general terms what follows from the ſame principles, if the common diviſor be $\overline{a-b}^r \times \overline{a-c}^s \times \overline{a-d}^t \times$ &c., then $r + 1$ roots of the given equation will be b, $s + 1$ roots will be c, $t + 1$ will be d, &c.; and it immediately follows, from the principles delivered in the ſecond edition of the ſame Book, publiſhed in 1770, that to find when $r + 1$, $v + 1$, $t + 1$, &c. roots are reſpectively equal requires $r + s + t$, &c. equations of condition, which are deducible from the well known method of finding the common diviſors of two quantities in this caſe of $a^n - pa^{n-1} + qa^{n-2} -$ &c., $na^{n-1} - \overline{n-1}pa^{n-2} + \overline{n-2}qa^{n-3} -$ &c. of the terms of their remainders, &c.

In the book above mentioned the equations of condition are given, which diſcover when two roots are equal in the equations $x^3 - px^2 + qx - r = 0$, $x^4 + qx^2 - rx + s = 0$, $x^5 + qx^3 - rx^2 + sx - t = 0$, in the two latter equations the ſecond term is wanting, which may eaſily be exterminated; but it may as eaſily be reſtored by ſubſtituting for q, r, s, &c. in the equation of condition found the quantities reſulting from the common transformation of equations to deſtroy the ſecond term.

2. Another rule contained in the ſame Book is the ſubſtitution of the roots of the equation $na^{n-1} - \overline{n-1}pa^{n-2} + \overline{n-2}qa^{n-3} - \&c. = 0$ reſpectively for a in the quantity $a^n - pa^{n-1} + qa^{n-2} - \&c.$, and multiplication of all the quantities reſulting into each other; their content will give the equation of condition, when two roots are equal.

Mr. HUDDE firſt diſcovered, that if the ſucceſſive terms of the given equation are multiplied into an arithmetical ſeries, the reſulting equation will contain one of any two equal roots, and m of the $\overline{m+1}$ equal roots in the given equation.

3. If 3, 4, 5, . . r roots of the equation are equal, find a common diviſor of 3, 4, 5, . . r of the ſubſequent quantities: $a^n - pa^{n-1} + qa^{n-2} - \&c.$, $na^{n-1} - \overline{n-1}pa^{n-2} + \overline{n-2}qa^{n-3} - \&c.$, $n.\overline{n-1}a^{n-2} - \overline{n-1}.\overline{n-2}pa^{n-3} + \overline{n-2}.\overline{n-3}qa^{n-4} - \overline{n-3}.\overline{n-4}ra^{n-5} + \&c.$, $n.\overline{n-1}.\overline{n-2}a^{n-3} - \overline{n-1}.\overline{n-2}.\overline{n-3}pa^{n-4} + \overline{n-2}.\overline{n-3}.\overline{n-4}qa^{n-5} - \&c.$, . . . $n.\overline{n-1}.\overline{n-2} \ldots \overline{n-r+2}a^{n-r+1} - \overline{n-1}.\overline{n-2} \ldots \overline{n-r+1}pa^{n-r} + \&c.$; which will probably be beſt done by dividing all the preceding quantities by the quantity of the leaſt dimenſion of a, and the diviſor and all the remainders by that quantity which has the leaſt dimenſions amongſt them; and ſo on: there will reſult 2, 3, 4, . . $r-1$ equations of condition; and in this caſe it is

obſerved, in the before-mentioned Book, that (if the common diviſor be $(a - A)$) it will once only admit of 3, 4, 5, . . r equal roots; if it be a quadratic, then it will twice admit of thoſe equal roots; and ſo on.

4. If the roots of the equation of the leaſt dimenſions be ſubſtituted for a in the remaining equations, and each of the reſulting values of the ſame equation be multiplied into each other, there will reſult the $r - 1$ equations of condition: and the ſame may be deduced alſo from the ſeveral equations conjointly.

The equations of conditions found by the firſt method, if the diviſions were not properly inſtituted, may admit of more rational diviſors than neceſſary, of which ſome are the equations of conditions required.

2.

1. In the year 1776, I publiſhed in the Meditationes Analyticæ a new method of differences for the reſolution of the following problem.

Given the ſums of a ſwiftly converging ſeries $ax + bx^2 + cx^3 + dx^4 + \&c.$, when the values of x are reſpectively π, ρ, σ, &c.; to find the ſum of the ſeries when x is τ, that is, given $S\pi = a\pi + b\pi^2 + c\pi^3 + d\pi^4 + \&c.$, $S\rho = a\rho + b\rho^2 + c\rho^3 + \&c.$, $S\sigma = a\sigma + b\sigma^2 + c\sigma^3 + \&c.$ &c.; to find $S\tau = a\tau + b\tau^2 + c\tau^3 + \&c.$

To reſolve this problem I multiplied the quantities, $S\pi$, $S\rho$, $S\sigma$, &c. reſpectively into unknown co-efficients α, β, γ, &c. and there reſulted

$a\pi\alpha$

$$\alpha\pi a+\alpha\pi^2 b+\alpha\pi^3 c+\&c.$$
$$\beta\rho a+\beta\rho^2 b+\beta\rho^3 c+\&c.$$
$$\gamma\sigma a+\gamma\sigma^2 b+\gamma\sigma^3 c+\&c.$$
$$\&c.\quad \&c.\quad \&c.$$

and then made the ſum of each of the terms reſpectively equal to its correſpondent term of the quantity $\tau a+\tau^2 b+\tau^3 c+\&c.$, and conſequently $\alpha\pi+\beta\rho+\gamma\sigma+\&c.=\tau$, $\alpha\pi^2+\beta\rho^2+\gamma\sigma^2+\&c.=\tau^2$, $\alpha\pi^3+\beta\varrho^3+\gamma\sigma^3+\&c.=\tau^3$, &c. I aſſumed as many equations of this kind as there were given values π, ϱ, σ, &c. of x; and conſequently as many equations reſulted as unknown quantities α, β, γ, &c.; whence, by the common reſolution of ſimple equations, or more eaſily from differences, can be found the unknown quantities α, β, γ, &c., and thence the equation ſought $\alpha\times S\pi+\beta\times S\varrho+\gamma\times S\sigma+\&c.=S\tau$ nearly.

3. In the Meditationes are aſſumed for π, ϱ, σ, &c. the quantities p, $2p$, $3p$, $4p$, $\ldots\overline{n-2}p$, $\overline{n-1}p$, and np for τ; which, if ſubſtituted for their values in the preceding equations, will give $\alpha+2\beta+3\gamma+4\delta+\&c.=n$, $\alpha+4\beta+9\gamma+16\delta+\&c.=n^2$, $\alpha+8\beta+27\gamma+\&c.=n^3$, $\alpha+16\beta+81\gamma+\&c.=n^4$; and if the ſums of the ſeries $ax+bx^2+cx^3+\&c.$ which reſpectively correſpond to the values p, $2p$, $3p$, $\ldots\overline{n-1}p$ of x be $S1$, $S2$, $S3$, $S4$, $\ldots S\overline{n-1}$, and the ſum of the ſeries $ax+bx^2+cx^3+\&c.$ which correſponds to n value of x be Sn; then will $Sn=nS\overline{n-1}-n.\frac{n-1}{2}S\overline{n-2}+n.\frac{n-1}{2}.\frac{n-2}{2}S\overline{n-3}\ldots\pm nS1$ nearly, which equation is given in the above-mentioned Book.

3. The logarithm from the number, the arc from the ſine, &c. are found by ſerieſes of the formula $ax+bx^2+cx^3+\&c.$; and conſequently this equation is applicable to them.

4. In the ſame Book is aſſumed a ſeries $ax^r + bx^{r+s} + cx^{r+2s} + dx^{r+3s} +$ &c. of a more general formula than the preceding, and in it for x ſubſtituted α, β, γ, δ, &c., m; and $S\alpha$, $S\beta$, $S\gamma$, $S\delta$, &c; Sm for the reſulting ſums, and thence deduced

$$Sm = \frac{m^r \times \overline{m^s-\beta^s} . \overline{m^s-\gamma^s} . \overline{m^s-\delta^s} . \&c.}{\alpha^r \times \overline{\alpha^s-\beta^s} . \overline{\alpha^s-\gamma^s} . \overline{\alpha^s-\delta^s} . \&c.} \times S\alpha + \frac{m^r \times \overline{m^s-\alpha^s} . \overline{m^s-\gamma^s} . \overline{m^s-\delta^s} . \&c.}{\beta^r \times \overline{\beta^s-\alpha^s} . \overline{\beta^s-\gamma^s} . \overline{\beta^s-\delta^s} . \&c.}$$

$$\times S\beta + \frac{m^r \times \overline{m^s-\alpha^s} . \overline{m^s-\beta} . \overline{m^s-\delta^s} . \&c.}{\gamma^r \times \overline{\gamma^s-\alpha^s} . \overline{\gamma^s-\beta^s} . \overline{\gamma^s-\delta^s} . \&c.} \times S\gamma + \frac{m^r \times \overline{m^s-\alpha^s} . \overline{m^s-\beta^s} . \overline{m^s-\gamma^s} \times \&c.}{\delta^r \times \overline{\delta^s-\alpha^s} . \overline{\delta^s-\beta^s} . \overline{\delta^s-\gamma^s} . \&c.}$$

$\times S\delta +$ &c. nearly.

Cor. If for r and s be aſſumed reſpectively 1, the ſeries becomes $ax + bx^2 + cx^3 +$ &c. of the ſame formula as the preceding: if $r = 0$ and $s = 1$, the ſeries becomes $a + bx + cx^2 +$ &c. The latter caſe will be the ſame as the former, when one of the quantities (α) ſubſtituted for x and its correſpondent ſum $S\alpha$, both become $= 0$, and the equation deduced in both caſes the ſame.

5. If π, ρ, σ, &c. reſpectively denote r, $r+p$, $r+2p$, .. $r+\overline{n-2}p$, $r+\overline{n-1}p$, and $\tau = r+np$; and S, $S1$, $S2$, $S3$, ..., $S\overline{n-2}$, $S\overline{n-1}$, be the ſums either reſulting from the ſeries $ax + bx^2 + cx^3 +$ &c. or the ſeries $A + ax + bx^2 + cx^3 +$ &c., which reſpectively correſpond to the values r, $r+p$, $r+2p$, &c. of x; and Sn the ſum of the ſame ſeries which correſponds to the value $r+np$ of x; then will $Sn = nS\overline{n-1} - n . \frac{n-1}{2} S\overline{n-2} + n . \frac{n-1}{2} . \frac{n-2}{3} S\overline{n-3} - \ldots \pm n . \frac{n-1}{2} S2 \mp nS1 \pm S$ nearly; this equation differs from the preceding by the laſt term S not vaniſhing; in the preceding caſe, S became $= 0$, for it was the ſum of the ſeries $ax + bx^2 + cx^3 +$ &c., which correſponded to $x = 0$.

6. From the Meditationes it appears, that $r^m - n \times \overline{r \pm p}^m + n \cdot \frac{n-1}{2} \overline{r \pm 2p}^m - n \cdot \frac{n-1}{2} \cdot \frac{n-2}{3} \overline{r \pm 3p}^m + \&c.$ to the end of the ſeries $= 0$, if m is leſs than n, and m and n are whole numbers; but if $m = n$, then it will $= \pm 1 \cdot 2 \cdot 3 \cdot 4 \ldots \overline{n-1} \cdot np^n$; whence it is manifeſt, that for the n firſt terms of the ſeries $A + ax + bx^2 + \&c.$ the equations are true; and for the $n-1$ firſt terms of the ſeries $ax + bx^2 + cx^3 + \&c.$ and in the ſucceſſive term of both the ſerieſes they will err by a quantity nearly $= \pm 1 \cdot 2 \cdot 3 \ldots n \times p^n \times r^{-n} \times$ co-efficient of the term; and the errors of every ſubſequent term (x^{b+n}) will be nearly as $\pm m \cdot \overline{\frac{m-1}{2}} \cdot \overline{\frac{m-2}{3}} \cdot \overline{\frac{m-3}{4}} \ldots \overline{\frac{m-b+1}{b}} \times p^n \times r^{-n} \times$ co-efficient of the term x^{b+n}, if for r, $r+p$, $r+2p$, &c. be ſubſtituted 1, $1+\frac{p}{r}$, $1+\frac{2p}{r}$, &c.

7. Let the preceding equation $Sn \pm nS\overline{n-1} - n \cdot \frac{n-1}{2} S\overline{n-2} + n \cdot \frac{n-1}{2} \cdot \frac{n-2}{3} \cdot S\overline{n-3} - \&c. = n \times \log. \overline{r-p} - n \cdot \frac{n-1}{2} \log. \overline{r-2p} + n \cdot \frac{n-1}{2} \cdot \frac{n-2}{3} \log. \overline{r-3p} + \&c. = \log. \frac{r \times \overline{r-2p}^{s} \times \overline{r-4p}^{s'} \times \overline{r-6p}^{s''} \times \&c.}{\overline{r-p}^{t} - \overline{r-3p}^{t'} \times \overline{r-5p}^{t''} \times \&c.} = \log. K$, where s, s', s'', &c. denote the co-efficients of the alternate terms of the binomial theorem, *viz.* $s = n \cdot \frac{n-1}{2}$, $s' = n \cdot \frac{n-1}{2} \cdot \frac{n-2}{3} \cdot \frac{n-3}{4}$, &c., and $t = n$, $t' = n \cdot \frac{n-1}{2} \cdot \frac{n-2}{3}$, &c. the co-efficients of the remaining alternate terms; the numerator $r \times \overline{r-2p}^{s} \times \overline{r-4p}^{s'} \times \overline{r-6p}^{s''} \times \&c. =$ (if $N = 2^{n-1}$) $r^N - Ppr^{N-1} + Qp^2r^{N-2} - Rp^3r^{N-3} \ldots Lp^{n-1} \times r^{N-n+1} \pm Mp^n r^{N-n} \mp \&c.$; and the denominator $\overline{r-p}^{t} \times \overline{r-3p}^{t'} \times \overline{r-5p}^{t''} \times \&c. = r^N - Ppr^{N-1} + Qp^2r^{N-2} -$

1

$Rp^3r^{n-3} + \ldots Lp^{n-1}r^{N-n+1}(\pm M + 1 \,.\, 2 \,.\, 3 \,.\,.\, \overline{n-1})\, p^n r^{N-n} \mp$ &c., whence the numerator and denominator have the n firſt terms the ſame, and the next ſucceeding terms differ by $1 \,.\, 2 \,.\, 3 \,.\,.\, \overline{n-1}\, p^n r^{N-n}$; the numerator divided by the denominator $= 1 \pm \frac{1 \,.\, 2 \,.\, 3 \,.\,.\, \overline{n-1}}{r^n}\, p^n$ nearly, if r be a great number in proportion to p, &c. it would be + when n is an odd number, and − when even.

8. The logarithm of the fraction K by the common ſeries $= K - 1 - \frac{\overline{K-1}^2}{2} + \frac{\overline{K-1}^3}{3} -$ &c. has for its firſt term $= \pm \frac{1 \,.\, 2 \,.\, 3 \,.\,.\, \overline{n-1}}{r^n} \times p^n$ nearly; for its ſecond term the ſquare of the firſt divided by 2, &c.

9. The error of this equation not only depends on the logarithm of K, which may be calculated to any degree of exactneſs, but in the calculus on the errors of the given logarithms.

10. If r be increaſed or diminiſhed by any given number, the n firſt terms of the numerator and denominator will ſtill reſult the ſame, and the next ſucceeding terms will differ by $1 \,.\, 2 \,.\, 3 \,.\, 4 \,.\,.\, n-1 \times p^n \times r^{N-n}$.

11. Let $n \,.\, \frac{n-1}{2}$ numbers be 2, $n \,.\, \frac{n-1}{2} \,.\, \frac{n-2}{3} \,.\, \frac{n-3}{4}$ numbers be 4, $n \,.\, \frac{n-1}{2} \,.\, \frac{n-2}{3} \,.\, \frac{n-3}{4} \,.\, \frac{n-4}{5} \,.\, \frac{n-5}{6}$ numbers be 6, &c.; their ſum, the ſum of the products of every two, the contents of every three, four, five, &c. to $n-1$ of them will be equal to the ſum, the ſum of the products of every two, of the contents of every three, four, five, &c. to $\overline{n-1}$ of the following numbers, *viz.* n numbers which are 1, $n \,.\, \frac{n-1}{2} \,.\, \frac{n-2}{3}$

numbers which are 3, $n . \frac{n-1}{2} . \frac{n-2}{3} . \frac{n-3}{4} . \frac{n-4}{5}$, which are 5, &c.; and the ſum of the contents of every n of the former will be leſs than the ſum of the contents of every n latter numbers by $1 . 2 . 3 . 4 . . \overline{n-1}$.

12. The method given in Art. 4. which I name a method of correſpondent values, eaſily deduces and demonſtrates the preceding equations, which cannot, without much difficulty, be done by the preceding method of differences; the method of correſpondent values is much preferable to the method of differences, both for the facility of its deduction, and the generality of its reſolution: for inſtance, from this method very eaſily can be deduced, &c. the ſubſequent and other ſimilar equations.

Ex. 1. $Sn = nS\overline{n-1} - n . \frac{n-1}{2} S\overline{n-2} + n . \frac{n-1}{2} . \frac{n-2}{3} S\overline{n-3} \ldots \pm nS1 \mp S$ nearly.

Ex. 2. $S\overline{n+m} = \frac{\overline{m+n} . \overline{m+n-1} . \overline{m+n-2} \ldots \overline{m+2}}{1 . 3 . . n-1} \times S\overline{n-1} - \frac{n-1}{1} \times A \times \frac{m+1}{m+2} \times S\overline{n-2} + \frac{n-2}{2} \times B \times \frac{m+2}{m+3} S\overline{n-3} - \frac{n-3}{3} \times C \times \frac{m+3}{m+4} \times S\overline{n-4} + \frac{n-4}{4} \times D \times \frac{m+4}{m+5} \times S\overline{n-5} -$ &c. nearly, where the letters A, B, C, D, &c. denote the preceding co-efficients, and the converging ſeries is the ſame as in the preceding example.

Ex. 3. Let the converging ſeries be of the formula $ax + bx^3 cx^5 + dx^7 +$ &c.; then will $Sn = \overline{2n-2}\, S\overline{n-1} - \overline{2n-1} \times \frac{2n-4}{2} S\overline{n-2} + \overline{2n-1} \times \frac{2n-2}{2} \times \frac{2n-6}{3} S\overline{n-3} - \overline{2n-1} . \frac{2n-2}{2} . \frac{2n-3}{2} \times \frac{2n-8}{4} S\overline{n-4} +$ &c. nearly, of which the general term is $\overline{2n-1}, \frac{2n-2}{2} . \frac{2n-3}{3} . . \frac{2n-l+1}{l-1} \times \frac{2n-2l}{l} \times S\overline{n-l}$

Ex. 4 Let the ſeries be of the formula $A + ax^2 + bx^4 + cx^6 +$ &c.; then will $Sn = \frac{n-1}{n} \times \overline{2n-2}\, S\overline{n-1} - \frac{n-2}{n} \times \overline{2n-1} \,.\, \frac{2n-4}{2} \times S\overline{n-2} + \frac{n-3}{n} \times \overline{2n-1} \,.\, \frac{2n-2}{2} \,.\, \frac{2n-6}{3} S\overline{n-3} - \frac{n-4}{n} \times \overline{2n-1} \,.\, \frac{2n-2}{2} \,.\, \frac{2n-3}{3} \times \frac{2n-8}{4} S\overline{n-4} +$ &c. nearly, of which the general term is $\frac{n-l}{n} \times \overline{2n-1} \,.\, \frac{2n-2}{3} \,.\, \frac{2n-3}{3} \ldots \frac{2n-l+1}{l-1} \times \frac{2n-2l}{l} \times S\overline{n-l}$.

Ex. 5. Let the given ſeries be of the formula $ax + bx^2 + cx^3 +$ &c., and in it for x be ſubſtituted p, $-p$, $2p$, $-2p$, $3p$, $-3p$, ..., np, $-np$ and mp, and for the ſums of the reſulting ſerieſes be wrote reſpectively S^1, S^{-1}, S^2, S^{-2}, S^3, S^{-3}, ... S^n, S^{-n}, and Sm; then will $Sm =$

$$\frac{m \,.\, \overline{m^2-1} \,.\, \overline{m^2-4} \,.\, \overline{m^2-9} \,.\, \overline{m^2-16} \ldots \overline{m^2-\overline{n-1}^2} \,.\, \overline{m-n}}{n \,.\, \overline{n^2-1} \,.\, \overline{n^2-4} \,.\, \overline{n^2-9} \,.\, \overline{n^2-16} \ldots \overline{n^2-\overline{n-1}^2} \times 2n = 1 \,.\, 2 \,.\, 3 \,.\, 4 \ldots 2n}$$

$\times S^{-n} + A \times \frac{m+n}{m-n} S^{+n} - \frac{2n}{1} \times B \times \frac{m-n}{m+n-1} S^{-n+1} - C \times \frac{m+n-1}{\overline{m-n-1}} \times S^{n-1} + \frac{2n-1}{2} \times D \times \frac{\overline{m-n-1}}{m+n-2} \times S^{-n+2} + \frac{m+n-2}{\overline{m-n-2}} \times E\, S^{n-2} - \frac{2n-2}{3} \times F \times \frac{\overline{m-n-2}}{m+n-3} S^{-n+3} - G \times \frac{m+n-3}{\overline{m-n-3}} \times S^{n-3} +$ &c. nearly, where the letters A, B, C, D, &c. reſpectively denote the preceding co-efficients. In general, the co-efficients of the terms S^{-n+s} and S^{n-s} will be reſpectively $M = \frac{2n-s+1}{s} \times L \times \frac{\overline{m-n-s+1}}{m+n-s}$ and $M \times \frac{m+n-s}{\overline{m-n-s}}$, where the letters L and M reſpectively denote their preceding co-efficients; the co-efficients are to be taken affirmatively, or negatively, according as s is an even or odd number.

Ex. 6. If for x in the preceding ſeries be ſubſtituted p, $-p$, $2p$, $-2p$, $3p$, $-3p$, ... $\overline{n-1}p$, $-\overline{n-1}p$, np reſpectively, then

$Sm = \frac{m\,.\,\overline{m^2-1}\,.\,\overline{m^2-4}\,.\,\overline{m^2-9}\,.\,\overline{m^2-16}\,\ldots\,(m^2-(n-1)^2)}{1\,.\,2\,.\,3\,.\,4\,.\,5\,\ldots\,(2n-1)}\,S^n - A \times \frac{m-n}{m+n-1} \times S^{-n+1} - \frac{2n-1}{1} \times B \times \frac{m+n-1}{m-n-1} \times S^{n-1} + C \times \frac{m-\overline{n-1}}{m+n-2} \times S^{-n+2} + \frac{2n-2}{2} \times D \times \frac{m+n-2}{m-n-2} \times S^{n-2} -$ &c. nearly, when A, B, C, &c. denote as before the preceding co-efficients. The coefficients of the terms S^{-n+s} and S^{n-s} will be reſpectively $L \times \frac{m-\overline{n-s+1}}{m+n-s}$ and $\frac{2n-1}{s} \times M \times \frac{m+\overline{n-s}}{m-n-s}$, L and M denoting the preceding co-efficients, which are to be taken negatively or affirmatively, as s is an even or an odd number. In this ſeries when $x=0$, the correſpondent ſum $=0$.

Ex. 7. Let the given ſeries be of the formula $a+bx+cx^2+dx^3+$ &c.; and in it for x be ſubſtituted $0, p, -p, 2p, -2p, 3p, -3p \ldots np, -pp$ and mp, and for the ſums of the reſulting ſerieſes be wrote as before S^0, S^1, S^{-1}, S^2, S^{-2}, ... S^n, S^{-n}, and S^m; then will $Sm = \frac{m\,.\,\overline{m^2-1}\,.\,\overline{m^2-4}\,.\,\overline{m^2-9}\ldots(m^2-\overline{n-1}^2)\times\overline{m-n}}{n\,.\,\overline{n^2-1}\,.\,\overline{n^2-4}\,.\,\overline{n^2-9}\ldots(n^2-\overline{n-1}^2)\times 2n} \times S^{-n} + A \times \frac{m+n}{m-n} \times S^{+n} - \frac{2n}{1} \times B \times \frac{m-n}{m+n-1} S^{-n+1} -$ &c. this ſeries obſerves the ſame law as the ſeries given in Ex. 5. and only differs from it by the laſt term $S0$ not vaniſhing, that is, being $=0$.

Ex. 8. Let the ſeries be of the preceding formula $a+bx+cx^2+dx^3+$ &c., and in it for x be ſubſtituted 0; $p, -p$; $2p, -2p$; $3p, -3p$; ... $\overline{n-1}p$, $-\overline{n-1}p$, np, and mp, and the ſums reſulting be $S0$, $S1$, S^{-1}, S^2, S^{-2}, .. $S^{\overline{n-1}}$, S^{-n+1}, S^n and S^m; then will $Sm = \frac{m\,.\,\overline{m^2-1}\,.\,\overline{m^2-4}\,.\,.\,\overline{m^2-(n-1)^2}}{1\,.\,2\,.\,3\,.\,4\ldots 2n-1} S^n - A \times \frac{m-n}{m+n-1} \times S^{-n+1} -$ &c. the ſame ſeries as in Ex. 6. and differs from it only by the laſt term $S0$ not vaniſhing.

Ex. 9. Let the ſeries be of the ſame formula $a+bx+cx^2+dx^3+$ &c. and in it for x be ſubſtituted p, $-p$, $3p$, $-3p$, $5p$, $-5p$, $7p$, $-7p$, ... np, $-np$ and mp; and the ſums reſulting be S^1, S^{-1}, S^3, S^{-3}, S^5, S^{-5}, S^7, S^{-7}, ... S^n, S^{-n}, and S^m; then will $S^m=$

$$\frac{\overline{m^2-1}\,.\,\overline{m^2-9}\,.\,\overline{m^2-25}\,.\,\overline{m^2-49}\,\ldots\,\overline{m^2-\overline{n-2}^2}\times\overline{m+n}}{\overline{n^2-1}\,.\,\overline{n^2-9}\,.\,\overline{n^2-25}\,.\,\overline{n^2-49}\,\ldots\,\overline{n^2-\overline{n-2}^2}^{\,2}\times 2n=2^n\times 1\,.\,2\,.\,3\,.\,4\,.\,5\ldots n}$$

$$\times S^n - A\times\frac{\overline{m-n}}{m+n}\times S^{-n}-\frac{n}{1}\times B\times\frac{m+n}{m-n+2}\times S^{n-2}+C\times\frac{m-n+2}{m+n-2}\times$$

$$S^{-n+2}+\frac{n-1}{2}\times D\times\frac{m+n-2}{m-n+4}\times S^{n-4}-E\times\frac{m-n+4}{m+n-4}S^{-n+4}-\frac{n-2}{3}F$$

$$\times\frac{m+n-4}{m-n+6}S^{n-6}+\frac{m-n+6}{m+n-6}\times G\times S^{-n+6}+\frac{n-3}{4}\times H\times\frac{m+n-6}{m-n+8}\times$$

$S^{n-8}-$ &c. nearly, where the letters A, B, C, D, E, &c. denote the preceding co-efficients of the terms S^n, S^{-n}, S^{n-2}, S^{-n+2}, S^{n-4}, S^{-n+4}, S^{n-6}, &c. reſpectively. The co-efficients of the terms S^{2n-2s} and S^{-n+2s} will be $M=\frac{n-s+1}{s}\times L\times\frac{m+n-2s+2}{m-n+2s}$ and $N=M\times\frac{m-n+2s}{m+n-2s}$; where L, M, and N denote the co-efficients of the terms immediately preceding each other, that is, of the terms $S^{-n+2s-2}$, S^{n-2s}, and S^{-n+2s}. The ſign of the firſt co-efficient M will be $+$ or $-$, according as s is even or odd; the ſecond term N will have a contrary ſign to the firſt.

Theſe ſeries may be made to begin from any term, which may be eaſily found by the method of correſpondent values, and the ſubſequent terms from it by the given law; its preceding terms may be deduced from the ſame law reverſed, that is, by putting the numerators of the fractions multiplied into it for the denominators, and the denominators for the numerators.

From theſe different ſerieſes may be formed, by adding two or more terms of the given ſeries together for a term of the required ſeries; which method has been applied to converging ſeries in general in the Meditationes.

13. The method of correſpondent values eaſily affords a reſolution of the problems contained in Mr. BRIGG's or Sir ISAAC NEWTON's method of differences.

EX. 1. Let the quantity be of the formula $a+bx+cx^2+dx^3 + \&c. \ldots x^n = y$, and $n+1$ correſpondent values of x and y be given, *viz.* p, q, r, s, &c. of x; Sp, Sq, Sr, Ss, &c. of y; then will

$$y = \frac{\overline{x-q}\,.\,\overline{x-r}\,.\,\overline{x-s}\,.\,\&c.}{\overline{p-q}\,.\,\overline{p-r}\,.\,\overline{p-s}\,.\,\&c.} \times Sp + \frac{\overline{x-p}\,.\,\overline{x-r}\,.\,\overline{x-s}\,.\,\&c.}{\overline{q-p}\,.\,\overline{q-r}\,.\,\overline{q-s}\,.\,\&c.} \times Sq + \frac{\overline{x-p}\,.\,\overline{x-q}\,.\,\overline{x-s}\,.\,\&c.}{\overline{r-p}\,.\,\overline{r-q}\,.\,\overline{r-s}\,.\,\&c.} \times Sr + \frac{\overline{x-p}\,.\,\overline{x-q}\,.\,\overline{x-r}\,.\,\&c.}{\overline{s-p}\,.\,\overline{s-q}\,.\,\overline{s-r}\,.\,\&c.} \times Ss + \&c.$$

The truth of this problem very eaſily appears by writing p, q, r, s, &c. for x in the given ſeries.

All the preceding examples may be applied to this caſe, by writing x for m in the given ſeries; hence the reſolutions of ſeveral caſes of equi-diſtant ordinates by eaſy and not inelegant ſerieſes, amongſt which are included the two caſes commonly given on this ſubject.

14. If a quantity be required, which proceeds according to the dimenſions of x, reduce the above given value of y into a quantity proceeding according to the dimenſions of x, and there reſults

$$y = \left(\frac{Sp}{\overline{p-q}\,.\,\overline{p-r}\,.\,\overline{p-s}\,.\,\&c. = A} + \frac{Sq}{\overline{q-p}\,.\,\overline{q-r}\,.\,\overline{q-s}\,.\,\&c. = B} + \frac{Sr}{\overline{r-p}\,.\,\overline{r-q}\,.\,\overline{r-s}\,.\,\&c. = C} + \frac{Ss}{\overline{s-p}\,.\,\overline{s-q}\,.\,\overline{s-r}\,.\,\&c. = D} + \&c.\right) \times x^n -$$

$$\left(\frac{Sp \times \overline{q+r+s+\&c.}}{A} + \frac{Sq \times \overline{p+r+s+\&c.}}{B} + \frac{Sr \times \overline{p+q+s+\&c.}}{C} + \frac{Ss \times \overline{p+q+r+\&c.}}{D} + \&c.\right) x^{n-1} + \left(\frac{Sp \times \overline{qr+qs+rs+\&c.}}{A} + \right.$$

$$\frac{Sq \times \overline{pr+ps+rs+\&c.}}{B} + \frac{Sr \times \overline{pq+ps+qs+\&c.}}{C} + \frac{Ss \times \overline{pq+pr+qr+\&c.}}{D} + \&c.)$$

$$\times\, x^{n-2} - \left(\frac{Sp \times \overline{qrs+\&c.}}{A} + \frac{Sq \times \overline{prs+\&c.}}{B} + \frac{Sr \times \overline{pqs+\&c}}{C} + \frac{Ss \times \overline{pqr+\&c.}}{D} + \right.$$

$\&c.)\, x^{n-3} + \&c.$

The law and continuation of this ſeries is evident to any one verſant in theſe matters from inſpection.

Theſe fractions may be reduced to a common denominator by ſubſtituting for Sp and A the products $Sp \times P$ and $A \times P$, where $P = \overline{q-r}\,.\,\overline{q-s}\,.\,\overline{r-s}\,.\ \&c.$; for Sq and B the products $Sq \times Q$ and $B \times Q$, where $Q = \overline{p-r}\,.\,\overline{p-s}\,.\,\overline{r-s}\,.\ \&c.$; for Sr and C the products $Sr \times R$ and $C \times R$, where $R = \overline{p-q}\,.\,\overline{p-s}\,.\,\overline{q-s}\,.\ \&c.$; for Ss and D the products $Ss \times S'$ and $C \times S'$, where $S' = \overline{p-q}\,.\,\overline{p-r}\,.\,\overline{q-r}\,.\ \&c.\ \&c.$

The fractions, in particular caſes, will often be reducible to lower terms.

15. Let $y = ax^b + bx^{b+l} + cx^{b+2l} + \&c.$, and the correſpondent values of x and y be given as before, then will $y =$

$$\frac{x^b \times \overline{x^l-q^l} \times \overline{x^l-r^l} \times \overline{x^l-s^l} \times \&c.}{p^b \times \overline{p^l-q^l} \times \overline{p^l-r^l} \times \overline{p^l-s^l} \times \&c.} \times Sp + \frac{x^b \times \overline{x^l-p^l} \times \overline{x^l-r^l} \times \overline{x^l-s^l} \times \&c.}{q^b \times \overline{q^l-p^l} \times \overline{q^l-r^l} \times \overline{q^l-s^l} \times \&c.} \times Sq$$

$$+ \frac{x^b \times \overline{x^l-p^l} \times \overline{x^l-q^l} \times \overline{x^l-s^l} \times \&c.}{r^b \times \overline{r^l-p^l} \times \overline{r^l-q^l} \times \overline{r^l-s^l} \times \&c.} \times Sr + \frac{x^b \times \overline{x^l-p^l} \times \overline{x^l-q^l} \times \overline{x^l-r^l} \times \&c.}{s^b \times \overline{s^l-p^l} \times \overline{s^l-q^l} \times \overline{s^l-r^l} + \&c.} \times$$

$Ss + \&c.$

This ſeries may in the ſame manner as the preceding be reduced to terms, proceeding according to the dimenſions of x; and the ſerieſes given in the examples may *(mutatis mutandis)* be predicated of it.

16. A more general method of correſpondent values is given in the Meditationes, as alſo the ſubſequent $y = \dfrac{\overline{x-q}\,.\,\overline{x-r}\,.\,\overline{x-s}\,.\,\&c.}{\overline{p-q}\,.\,\overline{p-r}\,.\,\overline{p-s}\,.\,\&c.}$

$\times \mathrm{S}p + \frac{\overline{x-p}\,.\,\overline{x-r}\,.\,\overline{x-s}\,.\,\&c.}{\overline{q-p}\,.\,\overline{q-r}\,.\,\overline{q-s}\,.\,\&c.} \times \mathrm{S}q + \frac{\overline{x-p}\,.\,\overline{x-q}\,.\,\overline{x-s}\,.\,\&c.}{\overline{r-p}\,.\,\overline{r-q}\,.\,\overline{r-s}\,.\,\&c.} \times \mathrm{S}r +$ &c. as in Ex. 1. $= \mathrm{S}p + (x-p)\left(\frac{1}{p-q} \times \mathrm{S}p + \frac{1}{q-p} \times \mathrm{S}q\right) + (x-p)$ $(x-q)\left(\frac{1}{p-q} \times \frac{1}{p-r} \times \mathrm{S}p + \frac{1}{q-p} \times \frac{1}{q-r} \times \mathrm{S}q + \frac{1}{r-p} \times \frac{1}{r-q} \times \mathrm{S}r\right) +$ $(x-p)(x-q)(x-r)\left(\frac{1}{p-q} \cdot \frac{1}{p-r} \cdot \frac{1}{p-s} \cdot \times \mathrm{S}p + \frac{1}{q-p} \cdot \frac{1}{q-r} \cdot \frac{1}{q-s} \times \mathrm{S}q + \frac{1}{r-p} \cdot \frac{1}{r-q} \cdot \frac{1}{r-s} \times \mathrm{S}r + \frac{1}{s-p} \cdot \frac{1}{s-q} \cdot \frac{1}{s-r} \times \mathrm{S}s\right) -$ &c.

The equality of theſe two different quantities will eaſily appear by finding the co-efficients of both, which are multiplied into the ſame given value of y as Sp, Sq, Sr, &c. and the ſame power of x; for with very little difficulty they will in general be found equal.

It is evident from this reſolution that, giving the ordinates and their reſpective diſtances from each other, the value of any other ordinate at a given diſtance from the preceding, found by this method, will reſult the ſame, whatever may be the point aſſumed from which the abſciſs is made to begin.

3.

1. Let a ſeries be $Ax + Bx^2 + Cx^3 + Dx^4 +$ &c. of ſuch a formula that if in it for x be ſubſtituted $a + b$, there reſults a ſeries $A \times \overline{a+b} + B \times \overline{a+b}^2 + C \times \overline{a+b}^3 + D \times \overline{a+b}^4 + \&c. = (Aa + Ba^2 + Ca^3 + Da^4 + \&c.) \times (1 + qb + rb^2 + sb^3 + tb^4 + \&c.) + (1 + qa + ra^2 + sa^3 + ta^4 + \&c.) \times (Ab + Bb^2 + Cb^3 + Db^4 + \&c.)$ then will the ſeries $Ax + Bx^2 + Cx^3 + Dx^4 + \&c. = Ax + \frac{2B}{1\,.\,2}x^2 + \frac{2\,.\,3C}{1\,.\,2\,.\,3}x^3 + \frac{24ABC - 8B^3}{1\,.\,2\,.\,3\,.\,4A^2}x^4 + \frac{36C^2A^2 + 24ACB^2 - 16B^4}{1\,.\,2\,.\,3\,.\,4\,.\,5A^3}x^5 + \frac{9\,.\,24A^2BC^2 - 4 \times 24AB^3C}{1\,.\,2\,.\,3\,.\,4\,.\,5\,.\,6A^4}x^6 + \frac{216C^3A^3 + 432A^2B^2C^2 - 384ACB^4 + 64B^6}{1\,.\,2\,.\,3\,.\,4\,.\,5\,.\,6\,.\,7A^5}x^7 + \&c.$;

$+$&c.; and the ſeries $1+qx+rx^2+sx^3+tx^4+$&c $= 1+\frac{B}{A}x+$

$$\frac{6CA-2B^2}{1.2A^2}x^2+\frac{18CAB-8B^3}{1.2.3A^3}x^3+\frac{36C^2A^2-8B^4}{1.2.3.4A^4}x^4+$$

$$\frac{180C^2A^2B-120ACB^3+16B^5}{1.2.3.4.5A^5}x^5+\frac{216C^3A^3+216A^2C^2B^2-288ACB^4+64B^6}{1.2.3.4.5.6A^6}x^6$$

$+$&c.

The terms of theſe two ſerieſes can eaſily be deduced by the ſubſequent method. Let $Kx^{n-2}+L^{n-1}+Mx^n$, be ſucceſſive terms of the ſeries $Ax+Bx^2+Cx^3+$&c., and $K^1x^{n-2}+L^1x^{n-1}$ ſucceſſive terms of the ſeries $1+qx+rx^2+sx^3+tx^4+$&c.; then will $M=\frac{2A^2\times B\times K^1+6CAK-2B^2K}{n.n-1\times A^2}$ and $L^1=\frac{n\times A\times M-B\times xL}{A^2}$.

Cor. 1. Let $B=0$, and the two ſerieſes $Ax+Bx^2+Cx^3+Dx^4+$&c. and $1+qx+rx^2+$&c. become reſpectively $Ax+\frac{2.3}{2.3}Cx^3+\frac{2^2.3^2}{2.3.4.5}\times\frac{C^2}{A}x^5+\frac{2^3.3^3}{2.3.4.5.6.7}\times\frac{C^3}{A^2}\times x^7+\frac{2^4.3^4}{2.3.4.5.6.7.8.9}\times\frac{C^4}{A^3}x^9+$&c., and $1+\frac{2.3}{1.2}\times\frac{C}{A}x^2+\frac{2^2.3^2}{1.2.3.4}\times\frac{C^2}{A^2}x^4+\frac{2^3.3^3}{1.2.3.4.5.6}\times\frac{C^3}{A^3}x^6+$&c.

If in theſe ſerieſes for A be ſubſtituted 1, and for C be ſubſtituted $-\frac{1}{2.3}$, there will reſult the ſerieſes $x-\frac{x^3}{2.3}+\frac{x^5}{2.3.4.5}-$&c., and $1-\frac{x^2}{1.2}+\frac{x^4}{1.2.3.4}-$&c. which give the ſine and coſine in terms of the arc x.

Cor. 2. Let $C=0$, and the above-mentioned ſeries $Ax+Bx^2+$&c. becomes $Ax+\frac{2}{1.2}Bx^2*-\frac{2^3}{1.2.3.4}\times\frac{B^3}{A^2}x^4-\frac{2^4}{1.2.3.4.5}\times\frac{B^4}{A^3}x^5*+\frac{2^6}{1.2.3.4.5.6.7}\times\frac{B^6}{A^5}\times x^7+\frac{2^7}{1.2.3.4...8}\times\frac{B^7}{A^6}x^8*-\frac{2^9}{1.2.3.4..10}\times\frac{B^9}{A^8}x^{10}-\frac{2^{10}}{1.2.3..11}\times\frac{B^{10}}{A^9}x^{11}+$&c. The law of this ſeries is, firſt, that every third term vaniſhes; and, ſecondly, the ſigns of every two ſucceſſive terms change alternately from + to − and − to +; and, laſtly, the co-efficient of the term x^n is $\frac{2^{n-1}}{1.2.3..n} \times \frac{B^{n-1}}{A^{n-2}}$; and the ſeries $1+qx+rx^2$ + &c. becomes $1+\frac{B}{A}x-\frac{2B^2}{1.2A^2}x^2-\frac{2^3B^3}{1.2.3A^3}x^3-\frac{2^3B^4}{1.2.3.4A^4}x^4+\frac{2^4B^5}{1.2.3.4.5A^5}x^5+\frac{2^6B^6}{1.2.3.4.5.6A^6}x^6+\frac{2^6\times B^7}{1.2.3..7A^7}x^7-$ &c. In this ſeries the ſigns of three ſucceſſive terms alternately change from + to − and − to +; and the co-efficient of the term x^n is $\frac{2^n\times B^n}{1.2.3.nA^n}$ or $\frac{2^{n-1}\times B^n}{1.2.3..nA^n}$ according as n is diviſible by 3 or not.

2. Let a ſeries $1+Px+Qx^2+Rx^3+Sx^4+Tx^5+$ &c. be of ſuch a formula, that if in it for x be ſubſtituted $a+b$, there reſults a ſeries $1+P\times\overline{a+b}+Q\times\overline{a+b}^2+R\times\overline{a+b}^3+S\times\overline{a+b}^4$ + &c. $=(1+Pa+Qa^2+Ra^3+Sa^4+$ &c.$)\times(1+Pb+Qb^2+Rb^3+Sb^4+$ &c.$)+(Aa+Ba^2+Ca^3+Da^4+$ &c.$)\times(Ab+Bb^2+Cb^3+Db^4+$ &c.), then will the ſeries $Ax+Bx^2+Cx^3+Dx^4+$ &c. $=Ax+Bx^2+\left(\frac{2B^2}{3A}-\frac{PB}{3}+A\times\frac{A^2+P^2}{6}\right)x^3+\left(\frac{2B^3-2PAB^2+A^2\times\overline{A^2+P^2}\times B}{6A^2}\right)x^4+$ &c., and the ſeries $1+Px+Qx^2+Rx^3+$ &c. $=1+Px+\frac{A^2+P^2}{2}x^2+\frac{2AB+P\times\overline{A^2+P^2}}{6}x^3+\frac{4B^2+\overline{A^2+P^2}^2}{24}x^4+$ &c. Let $Kx^{n-2}+Lx^{n-1}+Mx^n$ be ſucceſſive terms of the ſeries $Ax+Bx^2+Cx^3+$ &c., and $K'x^{n-2}+L'x^{n-1}+M'x^n$ ſucceſſive terms of the ſeries $1+Px+Qx^2+Rx^3+$ &c.; then will $A\times L+P\times L'=n\times M'$ and $B\times K+Q\times K'=n.\frac{n-1}{2}\times M'$ expreſs the law of the ſerieſes.

Cor.

Cor. Let B=0, then the ſeries $Ax+Bx^2+Cx^3+Dx^4=A\times$ $\left(x+\frac{P^2\times A^2}{2\cdot3}x^3+\frac{(P^2+A^2)^2}{1\cdot2\cdot3\cdot4\cdot5}x^5+\frac{(P^2+A^2)^3}{1\cdot2\cdot3\cdot4\cdot5\cdot6\cdot7}x^7+\&c.\right)$, and the ſeries $1+Px+Qx^2+Rx^3+\&c.=1+Px+\frac{P^2+A^2}{1\cdot2}x^2+P\times\frac{P^2+A^2}{1\cdot2\cdot3}x^3+\frac{(P^2+A^2)^2}{1\cdot2\cdot3\cdot4}x^4+P\times\frac{(P^2+A^2)^2}{1\cdot2\cdot3\cdots5}x^5+\frac{(P^2+A^2)^3}{1\cdot2\cdot3\cdot\cdot6}x^6+\&c.$; the co-efficient of the term x^n will be $(P^2+A^2)^{\frac{n}{2}}$ or $P\times(P^2+A^2)^{\frac{n-1}{2}}$, according as n is even or odd.

If in the equations before given for x be ſubſtituted $a=b$ inſtead of $a+b$, then in the other quantities for b ſubſtitute $-b$.

3. If in Caſe 2. the difference between the two quantities $(1+Pa+Qa^2+\&c.)\times(1+Pb+Qb^2+\&c.)$ and $(Aa+Ba^2+Ca^3+\&c.)\times(Ab+Bb^2+Cb^3+\&c.)$ is aſſumed $=1+P\times\overline{a+b}+Q\times\overline{a+b}^2+\&c.$, then in the ſerieſes before given for A, B, C, &c. write reſpectively $\sqrt{-1}A$, $\sqrt{-1}B$, $\sqrt{-1}C$, &c., and there will reſult the correſponding ſerieſes.

The ſame principles may be applied to many other caſes.

4. Equations of theſe formulæ may be uſeful, when the ſums of the ſerieſes correſpondent to a value (a) of x are given, and the ſums of the ſeries correſpondent to a value ($a+b$) of x is required, b having a ſmall ratio to a: for inſtance, let the given ſeries be $x-\frac{x^3}{2\cdot3}+\frac{x^5}{2\cdot3\cdot4\cdot5}-\frac{x^7}{2\cdot3\cdot\cdot7}+\&c.$; the equation found in the firſt caſe is $a+b-\frac{(a+b)^3}{2\cdot3}+\frac{(a+b)^5}{2\cdot3\cdot4\cdot5}-\&c.=\left(a-\frac{a^3}{2\cdot3}+\frac{a^5}{2\cdot3\cdot4\cdot5}-\&c.\right)\times\left(1-\frac{b^2}{1\cdot2}+\frac{b^4}{1\cdot2\cdot3\cdot4}-\&c.\right)+\left(1-\frac{a^2}{1\cdot2}+\frac{a^4}{1\cdot2\cdot3\cdot4}-\&c.\right)\times\left(b-\frac{b^3}{2\cdot3}+\frac{b^5}{2\cdot3\cdot4\cdot5}-\&c.\right)$;

but $a - \frac{a^3}{2 \cdot 3} + \frac{a^5}{2 \cdot 3 \cdot 4 \cdot 5} -$ &c., and $1 - \frac{a^2}{1 \cdot 2} + \frac{a^4}{2 \cdot 3 \cdot 4} -$ &c. are the ſine (s) and coſine (c) of an arc a of a circle whoſe radius is 1; and, conſequently, if the ſine s and coſine c of an arc a be given, the ſine of an arc $(a+b) = s \times (1 - \frac{b^2}{2} + \frac{b^4}{24} -$ &c.$) +$ $c(b - \frac{b^3}{2 \cdot 3} + \frac{b^5}{2 \cdot 3 \cdot 4 \cdot 5} -$ &c.), which ſeries, if b be very ſmall in proportion to a, converges much faſter than the common ſeries for finding the ſine from the arc: it has been given from different principles in the Meditationes, and is alſo eaſily deducible from the ſeries for finding the ſine and coſine from the arc by the propoſitions uſually given in plane trigonometry: the coſine of the ſame arc $(a+b) = c \times (1 - \frac{b^2}{1 \cdot 2} + \frac{b^4}{2 \cdot 3 \cdot 4} -$ &c.) $- s \times (b - \frac{b^3}{1 \cdot 2 \cdot 3} + \frac{b^5}{1 \cdot 2 \cdot \cdot 5} -$ &c.),

Ex. 2. Let the ſeries be $\overline{a+b} + \frac{\overline{a+b}^3}{2 \cdot 3} + \frac{\overline{a+b}^5}{2 \cdot 3 \cdot 4 \cdot 5} +$ &c. $=$ $(a + \frac{a^3}{2 \cdot 3} + \frac{a^5}{2 \cdot 3 \cdot 4 \cdot 5} +$ &c. $\times (1 + \frac{b^2}{1 \cdot 2} + \frac{b^4}{1 \cdot 2 \cdot 3 \cdot 4} +$ &c.$) +$ $(1 + \frac{a^2}{1 \cdot 2} + \frac{a^4}{1 \cdot 2 \cdot 3 \cdot 4} +$ &c.$) \times (b + \frac{b^3}{1 \cdot 2 \cdot 3} + \frac{b^5}{2 \cdot 3 \cdot 4 \cdot 5} +$ &c.); but $a + \frac{a^3}{1 \cdot 2 \cdot 3} +$ &c.$) = x$, and $1 + \frac{a^2}{1 \cdot 2} + \frac{a^4}{1 \cdot 2 \cdot 3 \cdot 4} +$ &c. $=$ $\sqrt{1+x^2}$, if a be the hyperbolic log. of $x + \sqrt{1+x^2}$; therefore $a + b + \frac{\overline{a+b}^3}{2 \cdot 3} + \frac{\overline{a+b}^5}{2 \cdot 3 \cdot 4 \cdot 5} +$ &c. $= x \times (1 + \frac{b^2}{2} + \frac{b^4}{2 \cdot 3 \cdot 4} +$ &c.$) +$ $\sqrt{1+x^2} \times (b + \frac{b^3}{2 \cdot 3} +$ &c.

Let $b + \frac{b^3}{2 \cdot 3} + \frac{b^4}{2 \cdot 3 \cdot 4 \cdot 5} +$ &c. $= y$, and $(x + \sqrt{1+x^2} \times$ $(y + \sqrt{1+y^2}) = V$, then will $\overline{a+b} + \frac{\overline{a+b}^3}{2 \cdot 3} + \frac{\overline{a+b}^5}{2 \cdot 3 \cdot 4 \cdot 5} +$ &c. $=$ $\overline{\tfrac{1}{2} V - \tfrac{1}{V}}$.

5. Let a quantity P be a function of x, or the fluent of a function of $x \times \dot{x}$, and the value X of it when $x=a$ be known, and the value of it when $x=a+b$ be required. Find a ſeries of which the firſt term is X, and which proceeds according to the dimenſions of b, if b be a very ſmall quantity, and in general at leaſt ſo ſmall that the ſeries from $x=a$ to $x=a+b$ neither becomes infinite or o.

In the ſame manner, if an algebraical or fluxional equation or equations, expreſſing the relations between x, y, z, v, &c. be given, find the correſpondent values of y, z, v, &c. to $x=a$, which let be Y, Z, V, &c.; then find ſerieſes for y, z, v, &c. of which the firſt terms let be Y, Z, V, &c. reſpectively, and which proceed according to the dimenſions of b, but ſubject to the ſame conditions as in the preceding caſe.

From fluxional equations may be deduced ſeries which expreſs the value of y, &c. in terms of x, and always diverge, or always converge, whatever may be its value, as appears from the Meditationes.

XVIII. *On the Reſolution of attractive Powers. By* Edward Waring, *M. D. F. R. S. and* Lucaſian *Profeſſor of Mathematics at* Cambridge.

Read May 28, 1789.

1. A FORCE acting at a given point may be reſolved by an infinite number of ways into two, three, or more (n) forces acting at the ſame point, either in the ſame or different planes with the given force and each other; and, *vice verſâ*, any number of ſuch forces acting in the ſame or different planes may be reduced into one.

Ex. Fig. 1. Tab. III. Let a body A be acted on by three forces AB, AC, and AD, not being in the ſame plane; reduce any two of them AB and AC to one AE, by compleating the parallelogram ABEC; then reduce the two forces AE and AD to one AF by completing the parallelogram AEFD, and the three forces AB, AC, and AD, are reduced to the one AF.

2. If n forces act on the body A at the ſame time, and any $(n-1)$ of them be reduced to one, the force reſulting will be ſituated in the ſame plane with the remaining, and force equivalent to the (n) forces.

3. If one force a be reſolved into ſeveral others $x, y, z, v,$ &c. ſituated in different planes, and the ſines of the angles, which the forces $y, z, v,$ &c. contain with the plane made by the direction of the forces x and a be reſpectively s, s', s'', &c. then will $sy \pm s'z \pm s''v \pm \&c. = 0$.

PRO-

PROBLEM I.

Fig. 2. Given the law of attraction of each of the parts of a given line in terms of their diſtance from a given point P; to find the attraction of the whole line *ab* on the point P.

Find the attraction of the line *ab* on the point P in the two directions P*f* and *fb* by the following method. Draw P*x* from the point P to any point *x* of the line *ab*, the force acting on the point P by the particle *xy* will be the given function (determined from the given law of attraction) of the diſtance into the particle; draw alſo P*h* perpendicular from the point P to the line *ab*, and let $Pf = a$, $hf = b$, and $fx = y$; then will the diſtance $Px = \surd(a^2 \pm 2by + y^2)$, and the function of the diſtance into the particle $xy = \varphi\, (\surd(a^2 \pm 2by + y^2)) \times \dot{y} = F\,(y) \times \dot{y}$; let this be denoted by *lx* ſituated in the line P*x*, which reſolve into two others $nx = \frac{y\dot{y} \times F\,:\,(y)}{Px = \surd\,(a^2 \pm 2by + y^2)}$ ſituated in the line *ab*, and *ln* (in a direction parallel to P*f*) $= \frac{a\dot{y} \times F\,:\,(y)}{\surd\,(a^2 \pm 2by + y^2)}$; find the fluents of the fluxions $\frac{y\dot{y} \times F\,:\,(y)}{Px}$ and $\frac{a\dot{y} \times F\,:\,(y)}{Px}$ contained between the values *af* and *fb* of the line $fx = y$, which ſuppoſe Y and V reſpectively; through the point *p* draw P*y* parallel to $fb = Y$, and in the line P*f* aſſume $Pu = V$; complete the parallelogram P*uzy*; Pz will be the force of the line *ab* on the point P.

Cor. If F : (*y*) varies as any power or root (2*n*) of the diſtance $Px = \surd(a^2 \pm 2by + y^2)$, and $n - \frac{1}{2}$ be an integer affirmative number or 0, the fluents Y and V of both the fluxions can be found in finite algebraical terms of *y*; if $n - \frac{1}{2}$ be an integer negative number, both the fluents can be found in the above-mentioned finite terms together with the arc of a circle, whoſe radius is $\sqrt{a^2 - b^2}$ and tangent $y \mp b$, unleſs $n - \frac{1}{2} = -1$, in which caſe the fluent Y involves that circular arc, and alſo the logarithm of $y^2 \pm 2by + a^2$. If $n - \frac{1}{2}$ denotes a fraction whoſe denominator is 2, both the fluents can be expreſſed by the finite terms together with the log. of $y \pm b + \sqrt{(y^2 \pm 2by + a^2)}$. If the fluents be given, when n is a given quantity, and $n - \frac{1}{2}$ not a whole affirmative number, from them can be deduced the fluents of any fluxions reſulting by increaſing or diminiſhing n by a whole number, unleſs in the above-mentioned caſe of $n - \frac{1}{2} = -1$. If $b = 0$, and conſequently the line Pf is perpendicular to the given line ab, the fluent Y will be expreſſed by the finite terms, unleſs $n - \frac{1}{2} = -1$, in which caſe it will be as $\frac{1}{2}$ log. $(y^2 + a^2)$ when properly corrected.

Theſe fluxions $\dot{Y}$ and $\dot{V}$ may be transformed into others, whoſe variable quantity is $Px = u$ the diſtance from P, by ſubſtituting in the fluxions for y and $\dot{y}$ their reſpective values $\sqrt{(u^2 - a^2 + b^2)} \mp b$ and $\frac{u\dot{u}}{\sqrt{(u^2 - a^2 + b^2)}}$, and conſequently for $\sqrt{(y^2 \pm 2by + a^2)}$ its value u.

PROBLEM II.

Fig. 3. Given the attraction of each of the parts of a given ſurface in terms of their diſtance from a given point P, and an equation expreſſing the relation between an abſciſs A$p = x$, and its correſpondent ordinates $pm = y$ of the ſurface; to find the attraction of the ſurface on the given point P.

Firſt, by the preceding propoſition find the attractions Y and V of any ordinate $m\,p\,m'$ in the directions of the ordinate pm and of the line Pp; and from the equation expreſſing the relation between the abſciſs and ordinates of the given curve, find the abſciſs in terms of the ordinates $(pm) = \pi : (y)$, and thence $\dot{x} = \varphi : (y) \times \dot{y}$ and $\sqrt{(a'^2 \pm 2sa'x + x^2)} = \varphi' : (y)$, where $PA = a'$ and $s =$ coſine of the angle, which the abſciſs A*p* makes with the line PA; then find the fluents of the three fluxions $\dot{x} \times Y = \dot{y} \times Y \times \varphi : (y)$, $\dot{x} \times \frac{V \times x}{\sqrt{(a'^2 \pm 2sa'x + x^2}} = \varphi : (y) \times \dot{y} \times \frac{\pi : (y)}{\varphi : (y)} \times V$ and $\dot{x} \times \frac{a'V}{\sqrt{(a'^2 \pm 2sa'x + x^2)}} = \dot{y} \times \frac{a'V}{\varphi' : (y)}$ contained between the values of *y*, which correſpond to the extreme values of *x*, which ſuppoſe Y′, V′, and Z; and draw through the point P the lines P*y* and P*z* reſpectively parallel to the ordinates *pm* and to the abſciſs A*p* and equal to $r \times Y'$ and V′; aſſume P*u* in the line $(PA) = t \times Z$, *r* and *t* denoting the ſines of the angles, which the ordinates *pm* and line AP make with the abſciſs A*p*: reduce theſe three forces P*y*, P*z*, and P*u*, to one P*f*, and P*f* will be the force of the ſurface on the point P.

Cor. 1. If for *y* and $\dot{y}$ be ſubſtituted their values in terms of *x* and $\dot{x}$, deduced from the equation expreſſing the relation between the abſciſs A*p* and ordinate *pm* of the given curve, thence will be deduced the above-mentioned fluents Y, V, Y′, V′, and Z, in terms of *x*; and in the ſame manner, if for *x* and $\dot{x}$ be ſubſtituted in the fluxions or fluents reſulting their values $\sqrt{(u^2 - a'^2 + 1^2 a'^2)} \mp sa'$, and its fluxion, there will reſult the above-mentioned fluxions or fluents in terms of *u* the diſtance from the point P.

Cor. 2. Let the curve be a circle, of which A is the center, PA a line perpendicular to the plane of the circle, and the ordinate *pm* perpendicular to the abſciſs A*p*; the forces on each ſide of the abſciſs A*p* will be equal, and the force in the direction of the abſciſs Ap will be equal to that in the contrary direction; the force in the direction (PA) $= 4 \times \int \frac{a\dot{u}}{\sqrt{(u^2 - a^2)}} \times \int \frac{u\dot{y}}{\sqrt{u^2 + y^2}} \times \mathrm{F} : (\sqrt{(u^2 + y^2)}) = \mathrm{W}$, in which $\mathrm{F} : (\sqrt{u^2 + y^2})$ is the function of the diſtance, according to which the given force on the particles varies; the fluent $\int \frac{u\dot{y}}{\sqrt{(u^2 + y^2)}} \times \mathrm{F} : \sqrt{(u^2 + y^2)}$ is contained between the values o and $\sqrt{(r^2 + a^2 - u^2)}$ of the quantity y, and the fluent W is contained between the values a and $\sqrt{(a^2 + r^2)}$ of the quantity u, where $a = \mathrm{PA}$ and r the radius of the circle; but the ſame force is $= 2 \times 3{,}14159$ &c. $\times \int a\dot{u} \times \mathrm{F} : (u)$, where $\mathrm{F} : (u)$ denotes the given function of the diſtance (u), and the fluent is contained between the values a and $\sqrt{a^2 + r^2}$ of u.

PROBLEM III.

To find the attraction of a given ſolid on a given point P. Find the attraction of every parallel ſection on that point by the preceding problem, and multiply it into the correſpondent fluxion of the firſt abſciſſa AP, and alſo find the fluent of the reſulting fluxion, which, properly corrected, multiply into the ſine of the angle, which the firſt abſciſſa makes with the parallel ſections, and the product will be proportional to the attraction of the ſolid on the given point P.

2. Fig. 4. Let the ſolid ABCH be generated by the rotation of a given curve round its axis AB, which paſſes through the point attracted P, and this ſolid be ſuppoſed to conſiſt of ſmall evaneſcent ſolids, whoſe baſes are the ſurfaces EF, ef, &c. of ſpheres, of which the center is P, and altitudes Ff, &c. the

increments of the baſe AB contained between the two contiguous ſurfaces EF and *ef*: from the points E and *e* of the curve draw ED and *ed* perpendicular to the axis AB, and ES perpendicular to the arc E*e* of the given curve at the point E, and meeting the axis AB in S; then will the evaneſcent ſolid EF*fe* $= p \times \mathrm{PE} \times \mathrm{FD} \times \mathrm{F}f = p \times \mathrm{FD} \times \mathrm{PS} \times \mathrm{D}d$ (becauſe $\mathrm{F}f = \frac{\mathrm{PS} \times \mathrm{D}d}{\mathrm{PE}}$) $= p \times \overline{\sqrt{(z^2 + y^2)} - z} \times \overline{z\dot{z} \mp y\dot{y}}$, where z and y denote reſpectively the abſciſs PD, and its correſpondent ordinate DE of the given curve.

The increment of the attraction of the ſurface EF on the point P in the direction PD will be as the increment of the ſurface $(p \times \mathrm{PE} \times \mathrm{D}d) \times \frac{\mathrm{PD}}{\mathrm{PE}} \times$ force of each particle $= p \times \mathrm{PD} \times \mathrm{D}d \times$ given force of the particle; but the fluent of the fluxion $\mathrm{PD} \times \mathrm{D}d$ contained between the points E and F is $= \frac{1}{2}\mathrm{PE}^2 - \frac{1}{2}\mathrm{PD}^2 = \frac{1}{2}\mathrm{ED}^2$, whence the attraction of the evaneſcent ſolid EF*fe* is as $\frac{1}{2}p \times \mathrm{ED}^2 \times \mathrm{F}f \times \mathrm{F} : (\sqrt{x^2 + y^2})$ force of each given particle at the diſtance $(\mathrm{PE} = \sqrt{(x^2 + y^2)}) = \frac{1}{2}p \times \mathrm{ED}^2 \times \frac{\mathrm{PS}}{\mathrm{PE}} \times \mathrm{D}d \times \mathrm{F} : (\sqrt{z^2 + y^2}) = \frac{1}{2}py^2 \times \frac{z\dot{z} \mp y\dot{y}}{\sqrt{(z^2 + y^2)}} \times \mathrm{F} : (\sqrt{(z^2 + y^2)})$; the fluent of which, properly corrected, is as the attraction of the ſolid on the point P; p denotes the circumference of a circle, whoſe radius is 1.

Cor. 1. The fluxion of this ſolid is $\frac{1}{2}py^2\dot{z} = \dot{\mathrm{Y}}$ which deduced from the preceding principles $= p \times (\sqrt{(z^2 + y^2)} - z) \times (z\dot{z} \mp y\dot{y}) = \dot{\mathrm{V}}$, and conſequently their fluents between two values of z, which correſpond to two values of $y = 0$, will be equal to each other.

Cor. 2. The increment of the attraction of this ſolid as given in this propoſition $\frac{1}{2}p \times y^2 \times \frac{z\dot{z} \mp y\dot{y}}{\sqrt{(z^2+y^2)}} \times F : (\sqrt{(z^2+y^2)}) = \dot{U}$, but in the preceding propoſition the force of a circle on the point $P = p \times \int a\dot{u} \times F : (u)$, where $u = \sqrt{(z^2+y^2)}$ and $a = z$, and y or u the only variable quantity contained in the fluxion; and conſequently the fluxion of the attraction of the ſolid $p \times \dot{z} \int z \frac{y\dot{y}}{\sqrt{z^2+y^2}} \times F : ((z^2+y^2)^{\frac{1}{2}}) = \dot{W}$; therefore, if for the fluent of $\frac{zy\dot{y}}{\sqrt{(z^2+y^2)}} \times F : ((z^2+y^2)^{\frac{1}{2}})$ be ſubſtituted its fluent contained between the values a and the value of y, which in the given equation correſponds to z; then the fluents of $\dot{U}$ and $\dot{W}$ contained between the two values of z, which correſponds to two values of $y = 0$, will be equal to each other.

The difference of the fluents of $\dot{Y}$ and $\dot{V}$, &c. contained between any other two values of z, can eaſily be deduced from the difference of two ſegments of ſpheres.

1. It may not be improper to remark in this place, that from different methods of finding the ſum of quantities, the fluents of fluxions, the integrals of increments, &c. quantities may often be deduced equal, which otherwiſe cannot without ſome difficulty; of which inſtances are contained in the Meditationes, and I ſhall here ſubjoin one or two more to thoſe already given in this Paper.

Ex. 1. Any curvilinear area ABC, &c. may be ſuppoſed to conſiſt of evaneſcent areas EF*ef*, of which the baſe EF is the arc of a circle, whoſe radius is $PE = \sqrt{(z^2+y^2)}$ and ſine $ED = y$, and altitude F*f*, and conſequently the fluxion of the area $= Ff \times$ arc (A) of a circle whoſe radius is PE and ſine ED =

PS

$\frac{PS}{PE}\times\dot{z}\times A=\frac{z\dot{z}\mp y\dot{y}}{\surd(z^2+y^2)}\times A=\dot{V}$; the fluent of $\dot{V}$ contained between the two values of z which correſpond to two values of $(y)=0$ will be equal to the fluent of $y\dot{z}$ contained between the ſame two values of z.

Ex. 2. The attraction of any circular arc EF in the direction PD on a point P (P being the center of a circle, of which EF is an arc, and ED the ſine of that arc) will be as ED × force at diſtance PE = ED × F : (PE); for the attraction in the direction PDll'F at the point x is as the increment of the arc $xy\times$ F : (PE) $\times\frac{Pl}{Px}$ (xl and yl' being at right angles to PF) $=U\times\frac{Px}{xl}\times\frac{Pl}{Px}\times F:(PE)=\frac{U'\times Pl}{lx}\times F:(PE)=\frac{u\dot{u}}{\surd\overline{PE^2-u^2}}\times F:(PE)$, if $u=Pl$; and conſequently the fluent of it is as $\surd\overline{PE^2-u^2}\times F:(PE)=ED\times F:(PE)$, and the attraction of the ſurface EFef will be as $ED\times Ff\times F:(PE)=ED\times\dot{z}\times\frac{PS}{PE}\times F:(PE)$ $=y\times\frac{z\dot{z}\mp y\dot{y}}{\surd(z^2+y^2)}\times F:((z^2+y^2)^{\frac{1}{2}}=\dot{V}$; the attraction of the curve will alſo vary as $\int\dot{z}\int\frac{z\dot{u}\times F:((u^2+z^2)^{\frac{1}{2}})}{(z^2+u^2)^{\frac{1}{2}}}=W$, in which the fluent of $\frac{z\dot{u}\times F:((z^2+u^2)^{\frac{1}{2}})}{(z^2+u^2)^{\frac{1}{2}}}$ is contained between $u=0$ and $u=y$; the fluents of $\dot{V}$ and $\dot{W}$ contained between two values of z, which correſpond to two values of $y=0$, will be equal to each other.

2. From a ſimilar method may be deduced equalities between other like fluents, for the curve may be ſuppoſed to conſiſt of other ſimilar curve ſurfaces equally as circles, and the ſolid of ſimilar ſegments of other ſolids equally as ſpheres.

3. From the ſame principles may innumerable ſerieſes equal to each other be deduced; for by different converging ſerieſes

find the ſum of the ſame quantity or quantities, and there will reſult ſerieſes equal to each other: for inſtance (fig. 5), if the time of falling down the arcs AC and BC and their interpolations from the principles delivered in the Meditationes Analyticæ, of which the difference let be D; find the difference between the times of a body's falling through BC when it began to fall from A and from B by a ſeries proceeding according to the dimenſions of $AB = o'$ a ſmall quantity; and find, by a ſeries of the ſame kind, the time of falling through AB; the ſum of theſe two ſerieſes will be equal to D. Similar propoſitions may be deduced from fluxional equations.

4. In ſome caſes the ratios of the times of bodies falling through ſome particular diſtances to each other may be eaſily known; for inſtance, let the force vary as the $m-1$ power of the diſtance (x), and a be the diſtance from which the body began to fall, then the velocity varies as $\surd(a^m - x^m)$, and the increment of the time as $\frac{\dot{x}}{\surd(a^m - x^m)}$; but if the parts of different curves are proportional, then will a, x, and $\dot{x}$ vary in the ſame ratio as each other, and conſequently the time through proportional parts of the diſtance will vary as $a^{\frac{2-m}{2}}$; and if the bodies be reſiſted likewiſe by a force which varies as the $\frac{2m-2}{m}$ power of the velocities, then the times through proportional parts will vary as before, that is, as $a^{\frac{2-m}{2}}$, where a denotes the proportional diſtances from the points where the forces and reſiſtances are equal.

PROBLEM IV.

1. Fig. 6. Given an equation expreſſing the relation between the two abſciſſæ $z = AP$ and $x = Pp$ and their correſpondent ordinates $y = pm$ of a ſolid, to find its ſolid contents contained between two values of its firſt abſciſſæ z. Aſſume z as an invariable quantity, and from the equation reſulting find the fluent Z of $y\dot{x}$ contained between the extreme values of x or y; then find the fluent of $Z\dot{z}$ contained between the given values of z, and the fluent multiplied into the product of the ſines of the angles, which the firſt abſciſſa makes with the plane of the ordinates and ſecond abſciſs, and the ſecond abſciſs makes with its correſpondent ordinates, will be the ſolid content required.

2. Fig. 7. Let the firſt abſciſs z of a ſolid be perpendicular to the planes of the ordinates, and the ſecond abſciſs $Pp = x$ perpendicular to the ordinates themſelves $pm = y$. Firſt, aſſume the firſt abſciſs as invariable, and find the increment of the arc $p'm = (\dot{x}^2 + \dot{y}^2)^{\frac{1}{2}}$, then aſſume the ſecond abſciſs Pp as conſtant, and let mu be the fluxion of the ordinate y or u, when the fluxion of the firſt abſciſs is $\dot{z} = ul$, where ul is perpendicular to the plane of the ordinates $p'pm$, and l a point of the ſurface of the ſolid; draw uh perpendicular to the arc $p'm$, and ſince ul is conſtituted at right angles to the plane $pp'm$, lh will cut the arc $p'm$ at right angles; but $uh = \frac{um \times pp'}{p'm} = \frac{\dot{u}\dot{x}}{\sqrt{(\dot{x} + \dot{y}^2)}}$, $lh = (hu^2 + lu^2)^{\frac{1}{2}} = \left(\frac{\dot{u}\dot{x}}{\dot{x}^2 + \dot{y}^2} + \dot{z}^2\right)^{\frac{1}{2}}$ the fluxion of the ſurface will be $lh \times \sqrt{(\dot{x}^2 + \dot{y}^2)}$. From the given equation expreſſing the relation between the two abſciſſæ z and x and ordinates y find, by aſſuming z invariable $p\dot{x} = \dot{y}$, and by aſſuming x invariable $q\dot{z} =$

$\dot{y}'=\dot{u}$, which being ſubſtituted for their values in the quantity $l'h \times \sqrt{(\dot{x}^2+\dot{y}^2)}$, there will reſult $(q^2+p^2+1)^{\frac{1}{2}} \times \dot{x} \times \dot{z} = A\dot{x}\dot{z} = \frac{(q^2+p^2+1)^{\frac{1}{2}}}{p} \times \dot{y} \times \dot{z} = B\dot{y}\dot{z}$; in A and B for y and x reſpectively ſubſtitute their value deduced from the given equation, and let the reſulting quantities be $A'\dot{x}\dot{z}$ and $B'\dot{y}\dot{z}$, where A' is a function of x and z, and B' a function of y and z; find the fluent of $A'\dot{x}\dot{z}$ from the ſuppoſition that x is only variable contained between the extreme values of x to a given value of z, which let be $L\dot{z}$, then find the fluent of $L\dot{z}$ by ſuppoſing z only variable contained between given values of z, and it will be the ſurface of the ſolid contained between thoſe values.

The ſame may be deduced by finding the fluent of $B'\dot{y}\dot{z}$ on the ſuppoſition that y is the only variable quantity contained between the extreme values of y as before of x to a given value of z, which let be $L'\dot{z}$; then will the fluent of $L'\dot{z}$ contained between the given values of z be the ſurface required.

If the ſolid be a cone generated by the rotation of a rectangular triangle round a ſide containing the right angle as an axis; hu will be a given quantity, if $\dot{z}$ be given.

If the above-mentioned angles are given, but not right ones, the arc $p'm$ and perpendicular $l'h$ can eaſily be deduced, and conſequently the increment of the ſurface.

3. To define a curve of double curvature, it is neceſſary to have two equations expreſſing the relation between the abſciſſæ z and x and their ordinates (y) given, and if the angles which they reſpectively make with each other be right ones; the fluxion of the arc as given in the *Proprietates Curvarum* is $(\dot{z}^2+\dot{x}^2+\dot{y}^2)^{\frac{1}{2}}$. Find its value from the two given equations in terms of x, y, or z, multiplied into its reſpective fluxions, and its fluent, properly corrected, will be the length of the arc required.

If the angles are not right, they may eaſily be reduced to them.

4. The attractions of theſe ſurfaces, curves, &c. on a given point P may be deduced from the preceding principles of finding the attractions of each of the parts in the directions of the firſt abſciſſa, which paſſes through the point P, the ſecond abſciſſa, and the ordinates, and then finding the integrals of theſe increments.

From the method which determines the attraction of a body, ſurface, &c. on a given point can be determined the attraction of a body, &c. on any number of points, and conſequently the attraction of one body, &c. on another, &c.

It is ſometimes advantageous to transform the firſt abſciſs, that it may paſs through the point attracted; and the abſciſſæ and ordinates, that they may be at right angles to each other, &c.

PROBLEM V.

1. Fig. 8. Given an equation expreſſing the relation between the two abſciſſæ AP and P*p* of a ſolid, and their correſpondent ordinates *pm*, or AP′, P′*p*′, and *p*′*m*′; to transform the firſt abſciſſa into any other L*h*.

Let the abſciſſa L*h* begin from a point L of the firſt abſciſſa AP, and meet an ordinate *pm* in the point *h*; draw *hp*, and let the ſines of the angles P*pm*, P*hp*, and *p*P*h*; LP*h*, P*h*L, and PL*h*, be denoted reſpectively by *r*, *s*, and *t*, &c. *r*′, *s*′, and *t*′; through a point *b* of the line P*h* draw *p*′*b*′*m*′ parallel to *pm*, and $Lh = z$, $hb' = x$, and $h'm' = y$: in the given equation for AP, P*p*, and *pm* ſubſtitute reſpectively their correſpondent values $\frac{s'z}{r'} \pm AL\,(a)$, $\frac{st'z}{rr'} \pm \frac{sx}{r}$ (for $Pb = \frac{t'z}{r'}$ and $Pb' = Pb \pm bb' = \frac{t'z}{r'} \pm x$), and $y \pm \frac{tt'z}{rr'} \pm \frac{tx}{r'}$; there reſults an equation to the ſame ſolid expreſſing the relation between the two abſciſſæ $z = Lb$ and x, and their correſpondent ordinates y.

1. 2. If the abſciſs Lb does not begin from L, a point in the firſt given abſciſs AP, but from M a point given out of it, it may be reduced to the preceding caſe, by drawing from M a line $MN = c$ to the plane of the firſt and ſecond abſciſſæ parallel to the ordinates pm; and from N to the firſt abſciſſa a line $NO = b$ parallel to the ſecond abſciſſæ, and ſubſtituting in the equation expreſſing the relation between AP, Pp, and pm for AP, Pp, and pm reſpectively $z \pm AO\ (a)$, $x \pm b$ and $y \pm c$; and there reſults the equation required expreſſing the relation between the two abſciſſæ z and x, and their correſpondent ordinates y, of which the firſt abſciſſa z paſſes through the point M.

2. To change the ſecond abſciſſa Pp into any other Lb, the firſt abſciſſa and ordinates remaining the ſame. In the preceding figure let L be conſidered as a moveable point of the firſt abſciſs AL, and the ſines of the reſpective angles denoted by the ſame letters as before, and $Lb = x$, $AL = z$, and $bm = y$; in the given equation for AP, Pp, and pm, ſubſtitute $z \pm \frac{s'x}{r'}$, $\frac{st'x}{rr'}$, and $y \pm \frac{tt'x}{rr'}$; and there will reſult the equation required expreſſing the relation between z and x the abſciſſæ, and their correſpondent ordinates y.

3. Fig 8. To change the ordinates, the abſciſſæ remaining the ſame, draw $p'm$ an ordinate transformed, $p'b$ parallel to the firſt abſciſſa AP, and meeting a ſecond abſciſſa, of which

pm is an ordinate in b: for the ſines of the angles $p'bp$, bpp', and $bp'p$; $p'pm$, pmp', and $pp'm$ write r, s, and t, r', s', and t'; and for AP′, P′p', and $p'm$ reſpectively z, x, and y; then ſubſtitute in the given equation for AP, Pp, and pm, their reſpective values $z\,(\mathrm{AP}') \pm \frac{ss'}{tr'} \times y$, $x\,(\mathrm{P}'p' \pm \frac{ts'}{rr'}y$, and $\frac{t'}{r'}y$; and there reſults an equation to the ſolid expreſſing the relation between the two abſciſſæ AP′ and P′p' and the tranformed ordinates $p'm$.

From theſe caſes, which are eaſily reducible to one, may be transformed any given abſciſſæ and their correſpondent ordinates into any other containing given angles, &c. with the before-mentioned abſciſſæ and ordinates.

In the properties of curve lines, firſt publiſhed in 1762, is given a method of deducing the equation to any ſection of the ſolid, and in particular the caſe of deducing the equation to the projection of any curve on a given plane.

From the principles given in this, and the Paper on centripetal forces, which the Royal Society did me the honour to print, can be deduced the fluxional equations, whoſe fluents expreſs the relations between the abſciſſæ and their correſpondent ordinates of the curves deſcribed by bodies, of which the particles act on each other with forces varying according to given functions of their diſtances.

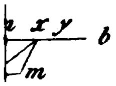
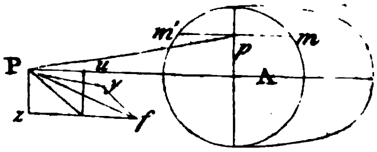


Fig. 3.



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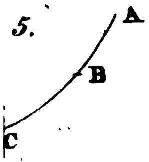

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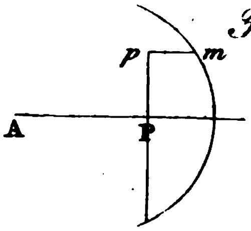

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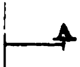
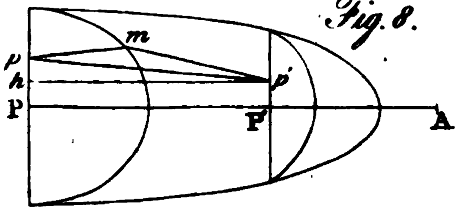


Fig. 8.



Basire Sc.

XIX. *Experiments on the Congelation of Quickſilver in* England. *By Mr.* Richard Walker; *in a Letter to* Henry Cavendiſh, *Eſq. F. R. S.*

Read May 28, 1789.

SIR,

I NOW beg leave to trouble you with the particulars of my experiments relative to the congelation of mercury; to which I ſhall add an account of a few experiments, relating to the production of artificial cold, made ſince my laſt Paper was written.

Exp. 1. On December 28th laſt, a favourable opportunity offered of beginning ſome experiments on the congelation of mercury, which I was deſirous of effecting completely; how far I have ſucceeded will appear in the ſequel.

For this purpoſe I prepared a mixture of diluted vitriolic acid (reduced by water till its ſpecific gravity was to that of water as 1,5596 to 1) and ſtrong fuming nitrous acid, of each equal parts. I preferred this mixture of acid becauſe it has been found by Mr. M^c^ NAB, in Hudſon's Bay, to be capable of producing much greater cold, when the temperature of the materials at mixing is very low, than the nitrous acid alone; the former ſinking a ſpirit thermometer to − 54°½, the latter never lower than − 46°.

The glaſs tube of a mercurial thermometer, with its bulb half filled with mercury, was provided; this occurring to me as a convenient method of aſcertaining when the mercury was congealed; for if, after being ſubjected to the cold of a frigorific mixture, the thermometer glaſs ſhould be taken out and inverted, and the mercury found to remain completely ſuſpended in that half of the bulb now uppermoſt, no doubt can remain of the ſucceſs of the experiment; an hydrometer, with its lower bulb half an inch in diameter, and three-fourths full of mercury, was likewiſe provided, in caſe any accident ſhould happen to the other.

It may be proper to premiſe here, that in all experiments of this kind I remove each veſſel, when the liquor it contains is ſufficiently cooled, out of the mixture in which it is immerſed for that purpoſe, immediately previous to adding the ſnow or ſalts with intention to generate a ſtill further increaſe of cold; and likewiſe prefer adding the ſnow or powdered ſalts to the liquor, inſtead of pouring the liquor upon theſe: it is neceſſary alſo to ſtir about the ſnow or ſalts, whilſt cooling in a frigorific mixture, from time to time, otherwiſe it will freeze into a hard maſs, and fruſtrate the experiment.

A half-pint glaſs tumbler, containing two ounces and a half of the above-mentioned diluted mixture of acids, being immerſed in mixtures of nitrous acid and ſnow, until the liquor it contained was cooled to $-30°$, was removed out of the mixture and placed upon a table; ſnow, likewiſe previouſly cooled in a frigorific mixture to $-15°$, was added by degrees to the liquor in the tumbler, and the mixture kept ſtirring until a mercurial thermometer ſunk to $-60°$, where it remained ſtationary; the hydrometer was then immerſed in the mixture (the thermometer glaſs having been broken in the courſe of the experiment), and ſtirred about in it for a ſhort time, and on taking the hydrometer out, and gently ſhaking it, I perceived the mercury had already acquired the conſiſtence of an amalgam, and after immerſing it again for a few minutes, and then taking out and inverting it, I was gratified for the firſt time with the ſight of mercury in a ſtate of perfect congelation. I applied my hand to the inverted glaſs bulb; this ſoon looſened the ſolid mercury, which, on ſhaking the hydrometer, was diſtinctly heard to knock with force againſt the glaſs; it was then immerſed a ſecond time, and when taken out was found adhering to the glaſs as before. I now inverted the glaſs again, and kept it in that ſituation until the whole of the mercury melted, and dropped down globule after globule into the ſtem of the hydrometer. The interval of time from taking the mercury out of the frigorific mixture in a ſolid ſtate, the laſt time, to its perfect liquefaction, was not noticed; but, upon recollection immediately afterwards, was ſuppoſed to be not leſs than three or four minutes. In a ſucceeding experiment this circumſtance was attended to, and the frozen mercury, weighing ſeven ſcruples, was not entirely melted under ſeven minutes, the temperature of the air + 30°.

The experiment which follows I conſider the moſt extraordinary, becauſe it proves beyond a doubt, that mercury may be frozen not only here in ſummer, but even in the hotteſt climate, at any ſeaſon of the year, by a combination of frigorific mixtures, in the way deſcribed in the Philoſophical Tranſactions, Vol. LXXVII. p. 285. in which attempt to freeze mercury, made April 20, 1787, the temperature of the air and materials being + 45°, I certainly reached (without the aſſiſtance of ſnow or ice) the point of mercurial congelation; but had then no ſatisfactory proof that any part of the mercury was abſolutely congealed.

Exp. 2. On December 30. three ounces of a mixture compoſed of ſtrong fuming nitrous acid two parts, and ſtrong vitriolic acid and water each one part, were cooled in a half pint tumbler immerſed in a frigorific mixture, till the temperature of the diluted mixture of acids was reduced to – 30°. The tumbler was then removed out of the mixture, and vitriolated natron (GLAUBER's ſalt) in very fine powder, previouſly cooled to – 14° by a frigorific mixture, added by degrees to the liquor in the tumbler, ſtirring it together until the mercury in the thermometer ſunk to – 54°. The hydrometer uſed in the former experiment, with its lower bulb three-fourths full of mercury, was now immerſed and ſtirred about in the mixture for a few minutes, when on taking it out, and inverting it, I had the ſatisfaction to find the ſame proof of the mercury being frozen as in the former inſtance. It was immediately ſhewn to the gentlemen preſent, who expreſſed likewiſe their entire ſatisfaction. Nearly four ounces of the powdered ſalt was added; but, I believe, ſome was added after the greateſt effect was produced. I had no nitrated ammonia by me, otherwiſe I ſhould have uſed upon this occaſion, inſtead of vitriolated natron alone, a mixture of theſe two ſalts in powder, in the proportion of ſeven parts of the former to eight of the latter. The temperature of the room in which theſe experiments were made was + 30° each time, and the mercury taken from a jar containing ſeveral pounds.

Exp. 3. By an experiment made purpoſely on January 10. laſt, at which Dr. BOURNE was preſent, I have found that mercury may be congealed tolerably hard, by adding freſh fallen ſnow, at the temperature of + 32°, to ſtrong fuming nitrous acid, previouſly cooled to between – 25° and – 30°, which may be very eaſily and quickly effected by immerſing the veſſel containing the acid in a mixture of ſnow and nitrous acid.

I uſe the *fuming* nitrous acid upon all occaſions, becauſe that does not require to be diluted, cold being immediately produced on the ſmalleſt addition of ſnow.

Exp. 4. On January 12, at Dr. Thomson's requeſt, I repeated the experiment of freezing mercury, at the Anatomy School in Chriſt Church, in the preſence of the honourable Mr. Wenman, the rev. Dr. Hoare, Dr. Sibthorp, junior, Dr. Thompson, the rev. Mr. Jackson of Chriſt Church, and Mr. Wood of this place, a gentleman well known for his ingenuity in mechanics.

For this purpoſe were provided a ſpirit thermometer graduated very low, and a mercurial thermometer graduated to −76°, two thermometer glaſſes, with bulbs very near, if not quite, an inch in diameter each, one filled with mercury nearly to the orifice of the tube, which was left open, the other with its bulb half filled, and an hydrometer with its lower bulb (conſiderably leſs than either of the others) likewiſe half filled with mercury; the temperature of the room at this time +28°.

A pan, containing nine ounces of the mixture of acids prepared as in the firſt experiment, was placed in a larger pan, containing nitrous acid, and this, in a frigorific mixture of nitrous acid and ſnow, contained in another pan much larger. When the nitrous acid in the ſecond pan was cooled by this mixture to −18°, and the mixed acids in the ſmalleſt pan nearly as much, ſnow at ſomewhat between +20° and +25°, the temperature of the open air at that time, was added to the nitrous acid in the ſecond pan, until the ſpirit thermometer ſunk to near −43°; then the thermometer, with its bulb half filled, was immerſed a ſufficient time, and when taken out, the mercury in it was found congealed, and adhering to the glaſs. The pan containing the mixed acids, and which had been removed whilſt the ſnow was added to make the ſecond mixture, was now replaced in it, in order to be cooled; and when the mixture of acids was reduced to the temperature of −34°, ſnow previouſly cooled to −18° was added, keeping the mixture ſtirred until the mercurial thermometer ſunk to −60°; its temperature by the ſpirit thermometer was then found to be −51°.

The three glaſſes containing the mercury to be frozen were now immerſed in this mixture, and having been moved about in it for a conſiderable time, during which the ſpirit thermometer roſe ſcarcely one degree, were then ſeverally taken out and examined.

As the examination of the frozen mercury was more immediately under the inſpection of Dr. THOMSON, I ſhall tranſcribe here that gentleman's account of the phænomena.

“When the freezing mixture was ſuppoſed to have produced its effect, the bulb which was completely filled was taken out, and broken on a flat ſtone by a moderate ſtroke or two with an iron hammer. This bulb was eleven or twelve lines in diameter.

The ſolid mercury was ſeparated into ſeveral ſharp and brilliant fragments, ſome of which bore handling for a ſhort time before they returned to a fluid form. One maſs, larger than the reſt, conſiſting of nearly one-third of the whole ball, afforded the beautiful appearance of flat plates, converging towards a center. Each of theſe plates was about a line in breadth at the external ſurface of the ball, becoming narrower as it ſhot inwards. Theſe facets lay in very different planes, as is common in the fracture of any cryſtallized ball, whether of a brittle metal or of the earths, as in balls of calcareous ſtalactite. The ſolid brittle mercury in the preſent inſtance bore a very exact reſemblance, both in colour and plated ſtructure, to ſulphurated antimony, and eſpecially to the radiated ſpecimens from Auvergne, before they are at all tarniſhed.

Inſtead of a ſolid center to this ball, it ſeemed as if there had been a central cavity, of about two lines in diameter, a conſiderable portion of which was evident in the fragment juſt deſcribed, at that part to which the radii converged. It is indeed poſſible, that this may have been merely the receptacle of ſome part of the mercury remaining fluid at the center. The hollow within was ſhining, but its edges were neither ſoft nor mouldering; on the contrary, they were ſharp and well defined: nor was the brilliancy of the radii attributable to any exudation of mercury as from an amalgam.

In the two ſmaller bulbs, which were only half filled, the mercury preſerved its uſual luſtre on the ſurface in contact with the glaſs, as well as on that ſurface which it had acquired in becoming ſolid. The latter was occupied by a conical depreſſion, the gradations of which were marked by concentric lines.

One of theſe hemiſpheres was ſtruck with a hammer, as in the former inſtance, but was rather flattened and cruſhed than broken. The other, on being divided with a ſharp chiſſel, ſhewed a metallic ſplendour on its cut ſurface, but not equalling the poliſh of a globule of fluid mercury."

Thirteen ounces of ſnow in the whole were found to have been added to the mixed acids; but ſome was added to lower its temperature after the glaſſes containing the mercury were taken out, and the ſpirit thermometer had riſen a few degrees.

This was a day remarkably favourable for ſuch an experiment. My thermometer expoſed to the open air ſtood, at three quarters paſt eight this morning, at + 6°, which is a very extraordinary degree of cold here; but this experiment was not begun till noon.

Exp. 5. On Jan. 14. I froze mercury at the Anatomy School again, in the preſence of the rev. the Dean of Chriſt Church, the rev. Dr. Hornsby, and Dr. Thomson.

Four ounces now of the mixture of acids, prepared as in the firſt experiment, were cooled in a tumbler to − 20°, which required ſomewhat more than an equal weight of ſnow, cooled nearly to the ſame temperature, to produce the greateſt effect. This was ſomewhat leſs than in the laſt experiment, the ſpirit thermometer ſinking no lower than − 46°, owing chiefly to the weather having become much warmer, the temperature of the open air being now + 36°. The mercurial thermometer immerſed in this mixture ſunk to − 55°, where it became ſtationary; then two thermometer glaſſes, one half filled with mercury, and the other filled to a conſiderable height up the the tube, after being immerſed ſome time, were examined. Upon breaking the ſhell of glaſs from the former of theſe, the mercury was found in a perfectly ſolid ſtate; but its upper ſurface, which was highly poliſhed, and of the colour of liquid mercury, inſtead of being only ſlightly depreſſed, as had been ſeen in every other inſtance which afforded an opportunity for inſpection, now formed a perfectly inverted hollow cone. This great depreſſion, as well as the concentric circles mentioned in a former inſtance, I ſuppoſe, might be owing to a rotatory motion accidentally given to it whilſt congealing. The ſolid mercury was beaten out, but having been ſuffered to lie ſome time on the table for inſpection, very quickly melted into liquid globules. The flexibility of ſolid mercury was clearly to be obſerved in this beautiful ſpecimen; for the external ſurface, particularly the upper thin rim of the concave part, was evidently bent by the firſt gentle ſtroke of the hammer. The globe of mercury in the other glaſs, which was very ſmall, exhibited nearly the ſame phænomena, as in the inſtances before mentioned.

It happened in theſe experiments of mine, contrary to what has generally occurred to others, that the mercury never ſunk lower than – 60°, ſeldom ſo low, in the thermometer, and but little below the point of mercurial congelation in the tubes of the thermometer glaſſes filled nearly up to the orifice, with a view to ſhew the contraction of mercury in becoming ſolid by its great deſcent in the tube. On reflecting on this circumſtance afterwards, it occurred to me, that the further deſcent of the mercury in theſe experiments was prevented not ſolely by the mercury freezing in the tube, the cauſe commonly aſſigned, but rather by the quick formation of a ſpherical ſhell of ſolid mercury within the bulb, by the ſudden generation of cold.

Dr. Beddoes expreſſing a deſire to exhibit ſolid mercury at his Lecture before his Claſs, I undertook to freeze ſome at the Laboratory on March 12th laſt, and now reſolved to ſatisfy myſelf reſpecting the cauſe which prevented the lower deſcent of the mercury in my former experiments. In this, as well as the former, the mercury in a thermometer graduated to – 60°, and likewiſe in a thermometer glaſs, filled nearly to the orifice, which lengthened its ſcale to near – 250°, ſunk only a few degrees below the point of mercurial congelation, and then remained ſtationary. After waiting ſome time, I took the thermometer out of the mixture, and obſerved the bulb apparently full, and the ſhort thread of mercury above unbroken.

I now

I now embraced the lower part of the tube with my hand a few ſeconds, reſting it upon the upper part of the bulb; and upon taking it away, I found that the whole of the mercury had ſubſided into the bulb, which it did not now quite fill, a ſmall ſpace at the top of the bulb remaining empty. I then took out the thermometer glaſs, and applied my hand to the tube; but the mercury remained ſtationary until I ſunk my hand ſo as to communicate heat to that part of the bulb which is immediately connected with the tube, when the thread of mercury dropped entirely into the bulb. It was now immerſed again for a ſhort time, then taken out, and the ſhell of glaſs beaten off, which expoſed a globe of ſolid mercury, nearly an inch in diameter. This bore ſeveral very ſmart ſtrokes with a hammer before it began to liquify, but was not perfectly malleable.

In the courſe of theſe experiments, ſeveral fragments of the ſolid mercury were thrown into mercury in its ordinary liquid ſtate, and were found to ſink with conſiderable celerity.

In continuing my reſearches reſpecting the means of producing artificial cold, I have found that phoſphorated natron produces rather more cold by ſolution in the diluted nitrous acid than the vitriolated natron.

At the temperature of +50°, four parts of the diluted nitrous acid (prepared by mixing ſtrong nitrous acid with half its weight of water) required eight parts of that neutral ſalt in fine powder to be added, in order to cauſe the thermometer to ſink to −6°; and again, by the addition of five parts of nitrated ammonia in fine powder, the thermometer ſunk ſo low as −16°, in the whole ſixty-ſix degrees.

A mixture of this kind made the thermometer ſink from 80° (the temperature of the materials before mixing) to 0°.

It was

I was directed to the trial of this ſalt, by the like remarkable ſenſation of coldneſs without pungency, which, with its other ſimilar properties to ice, firſt induced me, whilſt purſuing the ſubject of cold, to try the effect of diſſolving the vitriolated natron in the mineral acids.

Equal quantities, by weight, of phoſphorated natron and vitriolated natron, were evaporated ſeparately over a gentle fire, until each was reduced to a perfectly dry powder. I then weighed them, and found the reſiduum of the phoſphorated natron ſomewhat lighter than that of the vitriolated natron; from whence it is probable the former contains the greateſt quantity of water of cryſtallization.

I have found, that each of the neutral ſalts which produce any remarkable degree of cold by ſolution in the mineral acids, *viz.* phoſphorated natron, vitriolated natron, and vitriolated magneſia, loſe this property entirely, when deprived by any means of their water of cryſtallization.

A ſhort time after I had firſt ſucceeded in freezing water in ſummer, by one mixture compoſed of three different ſalts in water (having been induced to try the effect of ſuch a method, from the conſideration that water, already ſaturated with one kind of ſalt, will diſſolve a portion of another, and after that a third, or even more), I met with the account of an experiment made by M. HOMBERG, related in one of the earlier Volumes of the Philoſophical Tranſactions, in which it is ſaid he produced an extraordinary degree of cold, by pouring a pint and a half of diſtilled vinegar upon two pounds of a powder compoſed of equal parts of crude ſal ammoniac and corroſive ſublimate, and ſhaking them well together. I immediately (July 30, 1786) prepared a mixture of this kind in ſmaller quantity, but found it produced only thirty-two degrees of cold, the temperature of the air and materials before mixing being 63°; which is no more than I have found may be effected by a ſolution in water of crude ſal ammoniac alone, previouſly dried and powdered.

By a trial made with great accuracy, I find, that even the mixture compoſed of diluted vitriolic acid and vitriolated natron is adequate to any uſeful purpoſe that may be required in the hotteſt country; for, by adding eleven parts of the ſalt in fine powder to eight parts of the vitriolic acid diluted with an equal weight of water, the thermometer ſunk from 80°, the mean temperature of the hotteſt climate, and to which theſe materials were purpoſely heated before mixing, to rather below 20°.

Vitriolated natron, added to the marine acid undiluted, produces very nearly as great a degree of cold as when mixed with the diluted nitrous acid. At the temperature of 50°, two parts of the acid, require three parts of the ſalt in fine powder, which will ſink the thermometer to 0°; and if three parts of a mixed powder, containing equal parts of muriated ammonia and nitrated kali, be added afterwards, the cold of the mixture will be increaſed a few degrees more.

The frigorific mixture above deſcribed, compoſed of phoſphorated natron and nitrated ammonia diſſolved in the diluted nitrous acid, being the moſt powerful, it will probably be found moſt convenient for freezing mercury, when ſnow is not to be procured. The materials for this purpoſe may be previouſly cooled in mixtures made of marine acid with vitriolated natron, muriated ammonia, and nitrated kali, in the proportions mentioned above, this being much cheaper than thoſe made with diluted nitrous acid, and very nearly equal in effect.

In my laſt Paper I mentioned a freezing mixture, made by diſſolving a powder compoſed of equal parts of muriated ammonia and nitrated kali in water, and therein directed ſix parts of the mixed powder to be added to eight parts of water; but I have found ſince, that the beſt proportions are, five parts of the former to eight of the latter, by which I have ſunk the thermometer from 50° to 11°.

Having now proſecuted my ſubject relative to mixtures for generating artificial cold without the uſe of ice, from a poſſible method propoſed by Dr. WATSON (Eſſays, Vol. III. p. 139.), for freezing water in ſummer in this climate, and carried it on to a certain method of freezing, not only water, but even mercury, in the hotteſt climate, I now intend to take my leave of it.

I have the honour to be, &c.

RICHARD WALKER.

XX. *Catalogue of a ſecond Thouſand of new Nebulæ and Cluſters of Stars; with a few introductory Remarks on the Conſtruction of the Heavens. By* William Herſchel, *L L. D. F. R. S.*

Read June 11, 1789.

BY the continuation of a review of the heavens with my twenty-feet reflector, I am now furniſhed with a ſecond thouſand of new Nebulæ.

Theſe curious objects, not only on account of their number, but alſo in conſideration of their great conſequence, as being no leſs than whole ſidereal ſyſtems, we may hope, will in future engage the attention of Aſtronomers. With a view to induce them to undertake the neceſſary obſervations, I offer them the following catalogue, which, like my former one, of which it is a continuation, contains a ſhort deſcription of each nebula or cluſter of ſtars, as well as its ſituation with reſpect to ſome known object.

The form of this work, it will be ſeen, is exactly that of the former part, the claſſes and numbers being continued, and the ſame letters uſed to expreſs, in the ſhorteſt way, as many eſſential features of the objects as could poſſibly be crowded into ſo ſmall a compaſs as that to which I thought it expedient to limit myſelf.

The method I have taken of *analyzing* the heavens, if I may ſo expreſs myſelf, is perhaps the only one by which we can arrive at a knowledge of their conſtruction. In the proſecution of ſo extenſive an undertaking, it may well be ſuppoſed that many things muſt have been ſuggeſted, by the great variety in the order, the ſize, and the compreſſion of the ſtars, as they preſented themſelves to my view, which it will not be improper to communicate.

To begin our inveſtigation according to ſome order, let us depart from the objects immediately around us to the moſt remote that our teleſcopes, of the greateſt *power to penetrate into ſpace*, can reach. We ſhall touch but ſlightly on things that have already been remarked.

From the earth, conſidered as a planet, and the moon as its ſatellite, we paſs through the region of the reſt of the planets, and their ſatellites. The ſimilarity between all theſe bodies is ſufficiently ſtriking to allow us to comprehend them under one general definition, of bodies not luminous in themſelves, revolving round the ſun. The great diminution of light, when reflected from ſuch bodies, eſpecially when they are alſo at a great diſtance from the light which illuminates them, precludes all poſſibility of following them a great way into ſpace. But if we did not know that light diminiſhes as the ſquares of the diſtances encreaſe, and that moreover in every reflection a very conſiderable part is intirely loſt, the motion of comets, whereby the ſpace through which they run is meaſured out to us, while on their return from the ſun we ſee them gradually diſappear as they advance towards their aphelia, would be ſufficient to convince us that bodies ſhining only with borrowed light can never be ſeen at any very great diſtance. This conſideration brings us back to the ſun, as a refulgent fountain of light, whilſt it eſtabliſhes at the ſame time beyond a doubt that every ſtar muſt likewiſe be a ſun, ſhining by its own native brightneſs. Here then we come to the more capital parts of the great conſtruction.

Theſe ſuns, every one of which is probably of as much conſequence to a ſyſtem of planets, ſatellites, and comets, as our own ſun, are now to be conſidered, in their turn, as the minute parts of a proportionally greater whole. I need not repeat that by my analyſis it appears, that the heavens conſiſt of regions where ſuns are gathered into ſeparate ſyſtems, and that the catalogues I have given comprehend a liſt of ſuch ſyſtems; but may we not hope that our knowledge will not ſtop ſhort at the bare enumeration of phænomena capable of giving us ſo much inſtruction? Why ſhould we be leſs inquiſitive than the natural philoſopher, who ſometimes, even from an inconſiderable number of ſpecimens of a plant, or an animal, is enabled to preſent us with the hiſtory of its riſe, progreſs, and decay? Let us then compare together, and claſs ſome of theſe numerous ſidereal groups, that we may trace the operations of natural cauſes as far as we can perceive their agency. The moſt ſimple form, in which we can view a ſidereal ſyſtem, is that of being globular. This alſo, very favourably to our deſign, is that which has preſented itſelf moſt frequently, and of which I have given the greateſt collection.

But, firſt of all, it will be neceſſary to explain what is our idea of a cluſter of ſtars, and by what means we have obtained it. For an inſtance, I ſhall take the phænomenon which preſents itſelf in many cluſters: It is that of a number of lucid ſpots, of equal luſtre, ſcattered over a circular ſpace, in ſuch a manner as to appear gradually more compreſſed towards the middle; and which compreſſion, in the cluſters to which I allude, is generally carried ſo far, as, by imperceptible degrees, to end in a luminous center, of a reſolvable blaze of light. To ſolve this appearance, it may be conjectured, that ſtars of any given, very unequal magnitudes, may eaſily be ſo arranged, in ſcattered, much extended, irregular rows, as to produce the above deſcribed picture; or, that ſtars, ſcattered about almoſt promiſcuouſly within the fruſtum of a given cone, may be aſſigned of ſuch properly diverſified magnitudes as alſo to form the ſame picture. But who, that is acquainted with the doctrine of chances, can ſeriouſly maintain ſuch improbable conjectures? To conſider this only in a very coarſe way, let us ſuppoſe a cluſter to conſiſt of 5000 ſtars, and that each of them may be put into one of 5000 given places, and have one of 5000 aſſigned magnitudes. Then, without extending our calculation any further, we have five and twenty millions of chances, out of which only one will anſwer the above improbable conjecture, while all the reſt are againſt it. When we now remark that this relates only to the given places within the fruſtum of a ſuppoſed cone, whereas theſe ſtars might have been ſcattered all over the viſible ſpace of the heavens; that they might have been ſcattered, even within the ſuppoſed cone, in a million of places different from the aſſumed ones, the chance of this apparent cluſter's not being a real one, will be rendered ſo highly improbable that it ought to be intirely rejected.

Mr. Michell computes, with reſpect to the ſix brighteſt ſtars of the Pleiades only, that the odds are near 500000 to 1 that no ſix ſtars, out of the number of thoſe which are equal in ſplendour to the fainteſt of them, ſcattered at random in the whole heavens, would be within ſo ſmall a diſtance from each other as the Pleiades are *.

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* Phil. Tranſ. vol. LVII, p. 246.

Taking it then for granted that the ſtars which appear to be gathered together in a group are in reality thus accumulated, I proceed to prove alſo that they are nearly of an equal magnitude.

The cluſter itſelf, on account of the ſmall angle it ſubtends to the eye, we muſt ſuppoſe to be very far removed from us. For, were the ſtars which compoſe it at the ſame diſtance from one another as Sirius is from the ſun; and ſuppoſing the cluſter to be ſeen under an angle of 10 minutes, and to contain 50 ſtars in one of its diameters, we ſhould have the mean diſtance of ſuch ſtars twelve ſeconds; and therefore the diſtance of the cluſter from us about ſeventeen thouſand times greater than the diſtance of Sirius. Now, ſince the apparent magnitude of theſe ſtars is equal, and their diſtance from us is alſo equal,—becauſe we may ſafely neglect the diameter of the cluſter, which, if the center be ſeventeen thouſand times the diſtance of Sirius from us, will give us ſeventeen thouſand and twenty-five for the fartheſt, and ſeventeen thouſand wanting twenty-five for the neareſt ſtar of the cluſter;—it follows that we muſt either give up the idea of a cluſter, and recur to the above refuted ſuppoſition, or admit the equality of the ſtars that compoſe theſe cluſters. It is to be remarked that we do not mean intirely to exclude all variety of ſize; for the very great diſtance, and the conſequent ſmallneſs of the component cluſtering ſtars, will not permit us to be extremely preciſe in the eſtimation of their magnitudes; though we have certainly ſeen enough of them to know that they are contained within pretty narrow limits; and do not, perhaps, exceed each other in magnitude more than in ſome ſuch proportion as one full-grown plant of a certain ſpecies may exceed another full-grown plant of the ſame ſpecies.

If we have drawn proper concluſions relating to the ſize of ſtars, we may with ſtill greater ſafety ſpeak of their relative ſituations, and affirm that in the ſame diſtances from the center an equal ſcattering takes place. If this were not the caſe, the appearance of a cluſter could not be uniformly encreaſing in brightneſs towards the middle, but would appear nebulous in thoſe parts which were more crowded with ſtars; but, as far as we can diſtinguiſh, in the cluſters of which we ſpeak, every concentric circle maintains an equal degree of compreſſion, as long as the ſtars are viſible; and when they become too crowded to be diſtinguiſhed, an equal brightneſs takes place, at equal diſtances from the center, which is the moſt luminous part.

The next ſtep in my argument will be to ſhew that theſe cluſters are of a globular form. This again we reſt on the ſound doctrine of chances. Here, by way of ſtrength to our argument, we may be allowed to take in all round nebulæ, though the reaſons we have for believing that they conſiſt of ſtars have not as yet been entered into. For, what I have to ſay concerning their ſpherical figure will equally hold good whether they be groups of ſtars or not. In my catalogues we have, I ſuppoſe, not leſs than one thouſand of theſe round objects. Now, whatever may be the ſhape of a group of ſtars, or of a Nebula, which we would introduce inſtead of the ſpherical one, ſuch as a cone, an ellipſis, a ſpheroid, a circle or a cylinder, it will be evident that out of a thouſand ſituations, which the axes of ſuch forms may have, there is but one that can anſwer the phænomenon for which we want to account; and that is, when thoſe axes are exactly in a line drawn from the object to the place of the obſerver. Here again we have a million of chances of which all but one are againſt any other hypotheſis than that which we maintain, and which, for this reaſon, ought to be admitted.

The laſt thing to be inferred from the above related appearances is, that theſe cluſters of ſtars are more condenſed towards the center than at the ſurface. If there ſhould be a group of ſtars in a ſpherical form, conſiſting of ſuch as were equally ſcattered over all the aſſigned ſpace, it would not appear to be very gradually more compreſſed and brighter in the middle; much leſs would it ſeem to have a bright nucleus in the center. A ſpherical cluſter of an equal compreſſion within,—for that ſuch there are will be ſeen hereafter,—may be diſtinguiſhed by the degrees of brightneſs which take place in going from the center to the circumference. Thus, when a is the brightneſs in the center, it will be $\sqrt{a^2 - x^2}$ at any other diſtance x from the center. Or, putting $a = 1$, and $x =$ any decimal fraction; then, in a table of natural ſines, where x is the ſine, the brightneſs at x will be expreſſed by the coſine. Now, as a gradual encreaſe of brightneſs does not agree with the degrees calculated from a ſuppoſition of an equal ſcattering, and as the cluſter has been proved to be ſpherical, it muſt needs be admitted that there is indeed a greater accumulation towards the center. And thus, from the above-mentioned appearances, we come to know that there are globular cluſters of ſtars nearly equal in ſize, which are ſcattered evenly at equal diſtances from the middle, but with an encreaſing accumulation towards the center.

We may now venture to raiſe a ſuperſtructure upon the arguments that have been drawn from the appearance of cluſters of ſtars and nebulæ of the form I have been examining, which is that of which I have made mention in my “ *Theoreti-*

" *cal view—Formation of Nebulæ—Form I*[*]." It is to be remarked that when I wrote the paragraph I refer to, I delineated nature as well as I do now; but, as I there gave only a general ſketch, without referring to particular caſes, what I then delivered may have been looked upon as little better than hypothetical reaſoning, whereas in the preſent inſtance this objection is intirely removed, ſince actual and particular facts are brought to vouch for the truth of every inference.

Having then eſtabliſhed that the cluſters of ſtars of the 1ſt Form, and round nebulæ, are of a ſpherical figure, I think myſelf plainly authorized to conclude that they are thus formed by the action of central powers. To manifeſt the validity of this inference, the figure of the earth may be given as an inſtance; whoſe rotundity, ſetting aſide ſmall deviations, the cauſes of which are well known, is without heſitation allowed to be a phænomenon deciſively eſtabliſhing a centripetal force. Nor do we ſtand in need of the revolving ſatellites of Jupiter, Saturn, and the Georgium Sidus, to aſſure us that the ſame powers are likewiſe lodged in the maſſes of theſe planets. Their globular figure alone muſt be admitted as a ſufficient argument to render this point uncontrovertible. We alſo apply this inference with equal propriety to the body of the ſun, as well as to that of Mercury, Venus, Mars, and the Moon; as owing their ſpherical ſhape to the ſame cauſe. And how can we avoid inferring, that the conſtruction of the cluſters of ſtars, and nebulæ likewiſe, of which we have been ſpeaking, is as evidently owing to central powers?

Beſides, the ſtep that I here make in my inference is in fact a very eaſy one, and ſuch as ought freely to be granted. Have I not already ſhewn that theſe cluſters cannot have come to their preſent formation by any random ſcattering of ſtars? The doctrine of chance, by expoſing the very great odds againſt ſuch hypotheſes, may be ſaid to demonſtrate that the ſtars are thus aſſembled by ſome power or other. Then, what do I attempt more than merely to lead the mind to the conditions under which this power is ſeen to act?

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[*] Phil. Tranſ. vol. LXXV, p. 214.

In a caſe of ſuch conſequence I may be permitted to be a little more diffuſe, and draw additional arguments from the internal conſtruction of ſpherical cluſters and nebulæ. If we find that there is not only a general form, which, as has been proved, is a ſufficient manifeſtation of a centripetal force, what ſhall we ſay when the accumulated condenſation, which every where follows a direction towards a center, is even viſible to the very eye? Were we not already acquainted with attraction, this gradual condenſation would point out a central power, by the remarkable diſpoſition of the ſtars tending towards a center. In conſequence of this viſible accumulation, whether it may be owing to attraction only, or whether other powers may aſſiſt in the formation, we ought not heſitate to aſcribe the effect to ſuch as are *central*; no phænomena being more deciſive in that particular, than thoſe of which I am treating.

I am fully aware of the conſequences I ſhall draw upon myſelf in but mentioning other powers that might contribute to the formation of cluſters. A mere hint of this kind, it will be expected, ought not to be given without ſufficient foundation; but let it ſuffice at preſent to remark that my arguments cannot be affected by my terms: whether I am right to uſe the plural number,—central powers,—or whether I ought only to ſay,—the known central force of gravity,—my concluſions will be equally valid. I will however add, that the idea of other central powers being concerned in the conſtruction of the ſidereal heavens, is not one that has only lately occurred to me. Long ago I have entertained a certain theory of diverſified central powers of attractions and repulſions; an expoſition of which I have even delivered in the years 1780, and 1781, to the Philoſophical Society then exiſting at Bath, in ſeveral mathematical papers upon that ſubject. I ſhall, however, ſet aſide an explanation of this theory, which would not only exceed the intended limits of this paper, but is moreover not required for what remains at preſent to be added; and therefore may be given ſome other time, when I can enter more fully into the ſubject of the interior conſtruction of ſidereal ſyſtems.

To return, then, to the caſe immediately under our preſent conſideration, it will be ſufficient that I have abundantly proved that the formation of round cluſters of ſtars and nebulæ is either owing to central powers, or at leaſt to one ſuch force as refers to a center.

I ſhall now extend the weight of my argument, by taking in likewiſe every cluſter of ſtars or nebula that ſhews a gradual condenſation, or encreaſing brightneſs, towards a center or certain point; whether the outward ſhape of ſuch cluſters or nebulæ be round, extended, or of any other given form. What has been ſaid with regard to the doctrine of chance, will of courſe apply to every cluſter, and more eſpecially to the extended and irregular ſhaped ones, on account of their greater ſize: It is among theſe that we find the largeſt aſſemblages of ſtars, and moſt diffuſive nebuloſities; and therefore the odds againſt ſuch aſſemblages happening without ſome particular power to gather them, encreaſe exceedingly with the number of the ſtars that are taken together. But if the gradual accumulation either of ſtars or encreaſing brightneſs has before been admitted as a direction to the ſeat of power, the ſame effect will equally point out the ſame cauſe in the caſes now under conſideration. There are beſides ſome additional circumſtances in the appearance of extended cluſters and nebulæ, that very much favour the idea of a power lodged in the brighteſt part. Although the form of them be not globular, it is plainly to be ſeen that there is a tendence towards ſphericity, by the ſwell of the dimenſions the nearer we draw towards the moſt luminous place, denoting as it were a courſe, or tide of ſtars, ſetting towards a center. And—if allegoral expreſſions may be allowed—it ſhould ſeem as if the ſtars thus flocking towards the ſeat of power were ſtemmed by the crowd of thoſe already aſſembled, and that while ſome of them are ſucceſsful in forcing their predeceſſors ſideways out of their places, others are themſelves obliged to take up with lateral ſituations, while all of them ſeem equally to ſtrive for a place in the central ſwelling, and generating ſpherical figure.

Since then almoſt all the nebulæ and cluſters of ſtars I have ſeen, the number of which is not leſs than three and twenty hundred, are more condenſed and brighter in the middle; and ſince, from every form, it is now equally apparent that the central accumulation or brightneſs muſt be the reſult of central powers, we may venture to affirm that this theory is no longer an unfounded hypotheſis, but is fully eſtabliſhed on grounds which cannot be overturned.

Let us endeavour to make ſome uſe of this important view of the conſtructing cauſe, which can thus model ſidereal ſyſtems. Perhaps, by placing before us the very extenſive and varied collection of cluſters, and nebulæ furniſhed by my catalogues, we may be able to trace the progreſs of its operation, in the great laboratory of the Univerſe.

If theſe cluſters and nebulæ were all of the ſame ſhape, and had the ſame gradual condenſation, we ſhould make but little progreſs in this inquiry; but, as we find ſo great a variety in their appearances, we ſhall be much ſooner at a loſs how to account for ſuch various phænomena, than be in want of materials upon which to exerciſe our inquiſitive endeavours.

Some of theſe round cluſters conſiſt of ſtars of a certain magnitude, and given degree of compreſſion, while the whole cluſter itſelf takes up a ſpace of perhaps 10 minutes; others appear to be made up of ſtars that are much ſmaller, and much more compreſſed, when at the ſame time the cluſter itſelf ſubtends a much ſmaller angle, ſuch as 5 minutes. This diminution of the apparent ſize, and compreſſion of ſtars, as well as diameter of the cluſter to 4, 3, 2 minutes, may very conſiſtently be aſcribed to the different diſtances of theſe cluſters from the place in which we obſerve them; in all which caſes we may admit a general equality of the ſizes, and compreſſion of the ſtars that compoſe them, to take place. It is alſo highly probable that a continuation of ſuch decreaſing magnitudes, and encreaſing compreſſion, will juſtly account for the appearance of round, eaſily reſolvable, nebulæ; where there is almoſt a certainty of their being cluſters of ſtars. And no Aſtronomer can heſitate to go ſtill farther, and extend his ſurmiſes by imperceptible ſteps to other nebulæ, that ſtill preſerve the ſame characteriſtics, with the only variations of vaniſhing brightneſs, and reduction of ſize.

Other cluſters there are that, when they come to be compared with ſome of the former, ſeem to contain ſtars of an equal magnitude, while their compreſſion appears to be conſiderably different. Here the ſuppoſition of their being at different diſtances will either not explain the apparently greater compreſſion, or, if admitted to do this, will convey to us a very inſtructive conſequence; which is, that the ſtars which are thus ſuppoſed not to be more compreſſed than thoſe in the former cluſter, but only to appear ſo on account of their greater diſtance, muſt needs be proportionally larger, ſince they do not appear of leſs magnitude than the former. As therefore, one or other of theſe hypotheſes muſt be true, it is not all improbable but that, in ſome inſtances, the ſtars may be more compreſſed; and in others, of a greater magnitude. This variety of ſize, in different ſpherical cluſters, I am however inclined to believe, may not go farther than the difference in ſize, found among the individuals belonging to the ſame ſpecies of plants, or animals, in their different ſtates of age, or vegetation, after they are come to a certain degree of growth. A farther inquiry into the circumſtance of the extent, both of condenſation and variety of ſize, that may take place with the ſtars of different cluſters, we ſhall poſtpone till other things have been previouſly diſcuſſed.

Let us then continue to turn our view to the power which is moulding the different aſſortments of ſtars into ſpherical cluſters. Any force, that acts uninterruptedly, muſt produce effects proportional to the time of its action. Now, as it has been ſhewn that the ſpherical figure of a cluſter of ſtars is owing to central powers, it follows that thoſe cluſters which, *ceteris paribus*, are the moſt compleat in this figure, muſt have been the longeſt expoſed to the action of theſe cauſes. This will admit of various points of views. Suppoſe for inſtance that 5000 ſtars had been once in a certain ſcattered ſituation, and that other 5000 equal ſtars had been in the ſame ſituation, then that of the two cluſters which had been longeſt expoſed to the action of the modelling power, we ſuppoſe, would

would be moſt condenſed, and more advanced to the maturity of its figure. An obvious conſequence that may be drawn from this conſideration is, that we are enabled to judge of the relative age, maturity, or climax of a ſidereal ſyſtem, from the diſpoſition of its component parts; and, making the degrees of brightneſs in nebulæ ſtand for the different accumulation of ſtars in cluſters, the ſame concluſions will extend equally to them all. But we are not to conclude from what has been ſaid that every ſpherical cluſter is of an equal ſtanding in regard to abſolute duration, ſince one that is compoſed of a thouſand ſtars only, muſt certainly arrive to the perfection of its form ſooner than another, which takes in a range of a million. Youth and age are comparative expreſſions; and an oak of a certain age may be called very young, while a contemporary ſhrub is already on the verge of its decay. The method of judging with ſome aſſurance of the condition of any ſidereal ſyſtem may perhaps not improperly be drawn from the ſtandard laid down page 218; ſo that, for inſtance, a cluſter or nebula which is very gradually more compreſſed and bright towards the middle, may be in the perfection of its growth, when another which approaches to the condition pointed out by a more equal compreſſion, ſuch as the nebulæ I have called *Planetary* ſeem to preſent us with, may be looked upon as very aged, and drawing on towards a period of change, or diſſolution. This has been before ſurmiſed, when, in a former paper, I conſidered the uncommon degree of compreſſion that muſt prevail in a nebula to give it a planetary aſpect; but the argument, which is now drawn from the powers that have collected the formerly ſcattered ſtars to the form we find they have aſſumed, muſt greatly corroborate that ſentiment.

This method of viewing the heavens ſeems to throw them into a new kind of light. They now are ſeen to reſemble a luxuriant garden, which contains the greateſt variety of productions, in different flouriſhing beds; and one advantage we may at leaſt reap from it is, that we can, as it were, extend the range of our experience to an immenſe duration. For, to continue the ſimile I have borrowed from the vegetable kingdom, is it not almoſt the ſame thing, whether we live ſucceſſively to witneſs the germination, blooming, foliage, fecundity, fading, withering, and corruption of a plant, or whether a vaſt number of ſpecimens, ſelected from every ſtage through which the plant paſſes in the courſe of its exiſtence, be brought at once to our view?

WILLIAM HERSCHEL.

Slough near Windſor, May 1, 1789.

Firſt Claſs. Bright nebulæ.

I.	1785	Stars.		M.	S.		D.	M.	Ob	Deſcription.
94	April 28	61 Urſæ	f	0	6	n	2	17	2	cB. pL. E. ſp nf. vgmbM. 3½ l. 2′b.
95	—	—	f	35	0	n	2	7	2	cB. cL. E. np ſf. bM. 4′l. 3′b.
96	May 1	14 Cancri	f	5	30	n	1	12	2	vB. cL. mE. ſp nf. ſmbM. 6′l. 1′½ b.
97	—	—	f	7	58	n	0	47	1	vB. pL. E. nearly mer. gmbM.
98	—	—	f	36	50	ſ	0	12	1	cB. pL. R. vgmbM.
99	—	27 (γ) Bootis	p	13	46	ſ	1	46	2	vB. S. R. vſmbM.
100	Sept. 10	41 Ceti	f	13	43	n	0	48	1	cB. pS. R. mbM. See III. 431.
101	—	67 —	p	17	19	n	0	25	2	cB. pL. E. near. mer. mbM. 5′ l.
102	—	—	f	21	37	ſ	0	13	2	cB. pL. R. mbM.
103	24	14 Delphini	p	16	10	ſ	0	3	1	vB. L. gmbM. er. beautif. object.
104	28	93 (ψ) Aqua	f	1	8	n	0	42	1	cB. cL. E. near. mer. gmbM. F. rays.
105	Oct. 3	47 Ceti	f	26	24	ſ	0	37	1	cB. pL. iR. mbM.
106	—	89 (π) —	f	38	10	ſ	1	24	2	cB. cL. iR. bM. 3′ dia.
107	6	20 Eridani	f	4	3	ſ	1	4	2	vB. R. BNM. 1′½ dia.
108	8	111 (ξ) Piſcᵐ	p	34	22	ſ	0	1	1	cB. vL. iR. p. vBſt.
109	26	12 Eridani	p	7	17	n	2	54	3	cB. pS. lE. mer. mbM. r. 1′½ l.

I.	1785	Stars.		M.	S.		D.M.		Ob.	Deſcription.
110	Nov. 27	9 Ceti	p	44	0	f	0	47	2	cB. cL. lE. gmbM. iF.
111	—	— —	p	43	3	f	0	6	2	cB. cL. iR. gmbM.
112	29	5 (γ) Arietis	f	5	48	f	0	17	1	vB. L. R. mbM. not er. 4′ dia.
113	Dec. 7	60 (4th σ) Can	f	18	22	n	1	34	2	cB. cL. lE. iF. mb foll. ſide.
114	—	18 Leo. min.	p	13	39	f	0	35	1	cB. cL. iF. mbM.
115	—	— — —	p	5	47	n	1	10	2	cB. pL. lE. iF. mbM.
116 117	—	37 — —	f	11	5	n	1	1	1	Two; the 1ſt, cB. cL. iE; the 2d, pB. pL. iE. Diſt. 1′ at the vertex.
118	—	46 Urſæ	p	3	41	f	1	32	1	cB. cL. iR. mbM.
119	28	31 (1ſt d) Vir	p	6	0	n	0	55	1	vB. pS.
120	31	30 (α) Crateri	p	9	0	n	0	17	1	cB. L. iR. bM. 5′ l. 4′ b.
	1786									
121	Jan. 1	13 (η) Virgin	p	18	15	f	0	19	1	vB. cL. lE. mbM. 3′ l. 2′½ b. bet. 2 pBſt.
122	Feb. 1	57 (μ) Eridani	p	4	0	n	0	22	1	cB. vL. iR. bM. er. 5 or 6′ dia.
123	2	60 (σ) Virg.	p	52	27	f	0	30	2	cB. S.
124	—	— — —	p	39	57	f	0	3	2	cB. cL. R.
125	—	— — —	p	39	12	f	1	6	2	cB. cL. E. mbM.
126	24	108 —	p	0	35	n	1	15	1	cB. mE. par. BN. 8 or 9′ l.
127	—	110 —	p	1	47	f	0	23	1	cB. pS. mbM.
128	—	— —	f	3	37	f	0	30	1	vB. pL. bM.
129	March 3	26 (χ) —	f	9	46	f	0	41	1	v brilliant. iR. vgmbM.
130	—	— — —	f	26	35	f	0	3	2	vB. lE. mer. BN. and F. br. 2′ l.
131	4	14 (ι) Crate.	f	0	29	n	1	3	1	cB. E. gbM. 5′ l. 4′ b.
132	19	26 Hydræ	f	1	44	n	0	4	2	cB. pL. lE. vgbM. 1′½ diam.
133	25	49 (g) Virgin.	p	16	4	n	0	18	1	cB. vS. BN.
134	—	— — —	p	13	27	n	0	13	1	cB. 7 or 8′ l. 3′ b.
135 136	27	68 (ι) —	p	32	2	n	0	11	2	Two; both cB. cS. R. mbM. Diſt. 1′ near. mer. chev. mixed.
137	28	41 Lyncis.	f	3	13	n	0	8	1	vB. R. vſmbM. chev. 3′ dia.
138	—	*1 102 (ε) Hy	f	33	45	n	1	27	1	cB. R. pſmbM. — * See note.
139	April 17	11 (s) Virgin.	f	12	1	f	1	21	2	cB. vBN. r. 6 or 7′ dia.
140	—	— — —	f	39	55	f	0	31	2	cB. pL. mbM.
141	—	— — —	f	45	50	f	1	32	1	vB. cL. E. np ſf.
142	30	37 —	p	6	35	n	0	0	1	cB. pL. iR. gmbM.
143	—	43 (δ) Virgin	f	4	55	f	2	7	1	cB. np. pBſt. and cloſe to it.
144	—	109 —	p	25	58	n	0	54	1	cB. cL. R. gmbM.
145 146	—	— —	p	25	14	n	1	27	1	Two; the p. pB. pL. E. Diſt. 3 or 4′ ſp nf. The f cB. R. pL. Place of 2d.
147	—	43 Ophiuchi	p	8	54	f	1	17	1	vB. R. gmbM. 2′½ dia.
148	May 1	24 (α) Serpen	p	22	26	f	1	16	1	cB. cL. iR. bM.
149	28	40 (ξ) Ophiu	f	0	14	n	1	32	1	cB. pS. lE. er.
150	—	— — —	f	27	53	n	0	36	1	cB. R. vgmbM. about 1′½ dia.
151	Sept. 4	71 (ι) Piſcium	f	21	41	n	1	41	1	cB. cL. R. C. vgmbM. N.
152	—	24 (ξ) Arietis	p	16	23	n	0	20	2	vB. vS. R. or lE. vBN. 1′ ſf. cſt

I.	1786	Stars.		M.	S.		D.M.		Ob	Deſcription.
153	Sept. 20	59 (2d υ) Ceti	p	23	16	ſ	0	6	1	cB. vL. E. ſp nf. above 15′ l.
154	21	14 Triang.	f	1	23	n	0	59	2	cB pL. E. np ſf. vgmbM. 3′ l. 2′ b.
155	30	32 Eridani	f	7	49	ſ	1	1	2	cB. S. gmbM.
156	Oct. 18	12 (φ) Perſei	p	1	41	ſ	1	10	2	cB. mE. 12° ſp nf. vBN. near 10′ l.
157	26	90 (υ) Piſcium	f	28	9	n	0	13	1	cB. cL. E. par. mbM. 7′ l. 3′ b.
158	Nov. 26	48 (ν) Eridani	p	4	32	ſ	1	46	2	cB. pL. iR. vgmbM.
159	Dec. 11	20 (π) Caſſiop	f	8	30	n	0	33	3	vB. R. vgbM. 1′½ dia.
160	29	29 (γ) Virgin	p	6	17	ſ	2	19	2	vB. cL. E. ſp nf. vgBN. F. bran.
	1787									
161	Jan. 14	6 Comæ	f	12	58	ſ	0	55	1	vB. pL. iR.
162	—	29 —	f	10	35	n	0	2	1	vB. E. ſp nf. Sſt in it ½′ p. N.
163	Feb. 22	20 Sextantis	p	8	29	ſ	0	22	1	cB. cL. mE. 45° ſp nf. N. 2′ l. F. br. 5′ l.
164	Mar. 17	38 Leo. min.	p	2	54	ſ	0	36	3	cB. E. 30° np ſf. mbM. er. 4′ l. 2′ b.
165	—	6 Canum	p	15	42	n	0	25	2	vB. BN. not M. or 2 joined the n. N.
166	—	— —	p	1	20	n	0	23	2	vB. S. R. mbM.
167	18	10 (κ) Urſæ	f	13	43	ſ	1	40	1	cB. R. BN. 1′½ dia.
168	—	34 (μ) —	p	4	9	ſ	0	6	3	cB. R. vgbM. 8′ dia. eſt. in it, unconnected.
169	—	6 Canum	p	16	16	n	0	53	1	cB. cL.
170	—	20 —	f	28	12	n	1	6	2	cB. E. near par. SNM. 2′ l.
171	—	53 (2d ν) Boot	p	49	57	n	1	10	2	cB. S. R. r. mbM.
172	19	31 Leo. min.	f	25	2	ſ	0	3	1	cB. E. ſp nf. few ſt. in p. 1 in n. unconnected.
173	—	— —	f	36	19	n	0	23	1	vB. R. vgNM. 2′½ dia.
174	20	53 (ξ) Urſæ	f	46	14	n	0	24	1	cB. E. 5′ l. 1′½ b.
175	—	13 Canum	p	46	3	n	2	28	1	vB. S R. mbM.
176 177	—	— —	p	16	33	n	1	26	1	Two. The ſ. cB. E. mbM. The n. pB. E. ſp nf. Both join and form the letter S.
178 179	April 9	8 —	f	7	36	ſ	0	12	1	Two. The n. vB. vmbM. The ſ. pB. Their nebul. run together.
180	—	20 —	f	29	9	n	3	15	1	cB. mE. 60° np ſf. vſBM.
181	—	— —	f	40	13	n	1	11	1	cB. cL. mbM.
182	11	1 Serpentis	p	17	22	ſ	0	2	2	cB. pL. iR. mbM.
183	—	— —	p	11	19	n	0	1	2	cB. pL. iR. or lE.
184	May 7	8 Libræ	p	8	21	ſ	1	15	1	cB. pL. E. ſp nf. mbM.
185	11	19 (λ) Bootis	f	11	6	n	0	1	2	c or pB S. R. pſmbM.
186	12	— — —	p	47	14	n	1	20	2	cB. pL. R. or lE. vgbM. 3′ np. the 51ſt of the *Conn. des Temps.*
187	—	— —	p	20	15	n	1	14	1	cB. E. 30° ſp nf. BN. vgF. branches.
188	—	38 (2d *b*) —	p	13	24	n	2	44	2	cB. lE. par. mbM. F. bran. 1′½ l.
189	15	24 (*g*) —	f	3	57	ſ	0	23	1	cB. cL. E. ſp nf. broad.

I.	1787	Stars.		M. S.		D. M.	Ob.	Deſcription.
190 191	May 16	*Canum 6m.	f	11 32	ſ	1 11	1	Two. The ſ. cB. cL. The n. pB. S. diſt. 1′½. * See note.
192	Oct. 14	3 Lacertæ	p	80 46	n	2 32	3	cB. iF. 3′ l. 2′½ b. Nebuloſity.
193	Nov. 12	54 (φ) Andro	p	1 26	n	0 54	1	Two cloſe together. Both vB. diſt. 2′. ſp nf. One is 76 of the *Conſ.*
	1788							
194	Jan. 14	56 Urſæ	f	3 19	n	0 5	2	vB. cL. mE. mer. BN. 6′ l. 2′ b. chev.
195	—	67 ——	f	4 49	n	0 2	2	E. vBN. and F. branches.
196	—	— ——	f	7 17	n	0 38	2	cB. cL. iF. vgbM. ſf. ſt.
197 198	—	8 Canum	p	3 32	n	0 19	1	Two. The ſ. vB. vL. iE. The n. B. pS. iF. diſt. 1′½.
199	15	15 Leo. min.	f	32 1	ſ	0 24	2	cB. mE. ſp nf. vgbM. 5′ l. 2 or 3′ b.
200	Feb. 5	59 (2d σ) Can	p	4 29	n	0 29	1	v brilliant. mE. ſp nf. 8′ l. 3′ b. beauti.
201	—	63 (χ) Urſæ	f	0 5	ſ	0 17	2	cB. mE. ſp nf. near. mer. 5′ l. 1′ b.
202	—	— — ——	f	0 47	n	0 4	2	cB. S. lE.
203	6	59 ——	f	7 42	n	0 31	1	cB. cL. R. pBNM.
204	March 9	9 (ι) ——	p	16 27	n	2 7	1	cB. vS. lE. m.
205	—	— — ——	f	22 18	n	3 1	1	vB. lbM. chev. bran. m. neb. 6′ l. 4′ b.
206	—	3 Canum	p	14 39	n	1 35	3	cB. E. 45° np ſf. 6′ l. 4′ b. almoſt equally B.
207	—	— ——	p	14 0	ſ	1 32	3	cB. mE. 70° ſp nf. 6 or 7′ l. 2′ b.
208	—	— ——	p	9 9	n	1 32	3	cB. mE. ſp nf. SBNM. 5′ l. 1′ b.
209	—	— ——	p	3 33	ſ	1 6	2	cB. cL. E. mbM.
210	April 1	60 Urſæ	f	46 0	n	0 9	2	vB. S. lE. near par. BN. eF. bran.
211	—	11 Canum	f	5 47	ſ	1 58	3	cB. S. R. bM. f. vSſt.
212	10	60 Urſæ	f	50 50	ſ	1 58	1	cB. pL. E.
213	27	19 (λ) Boötis	p	110 25	ſ	1 48	1	v brilliant. cL. E. ſp nf. difficulty r. has 3 or 4 BN.
214	May 1	17 (κ) ——	p	8 26	n	1 56	2	cB. cL. n. ends abruptly. f. vg.
215	5	Neb. II. 757.	p	3 27	ſ	1 14	1	vB. cL. E. f. 2 ſt.

Second claſs. Faint nebulæ.

II.	1785	Stars.		M. S.		D. M.	Ob.	Deſcription.
403	April 26	1 Comæ	p	8 50	ſ	1 21	3	F. cL. iF. lbM.
404	27	5 ——	p	11 40	ſ	0 29	1	pB. pL. R. C. mbM.
405	—	— ——	p	1 0	ſ	0 24	2	pB. pL. iF. lE. bM. p. pcſt.
406	—	20 ——	p	6 8	ſ	1 27	1	pF. pL. mbM. S neb. joined to it. or lb. in the n.
407	—	— ——	f	6 44	ſ	1 35	1	pB. pS. lE.
408	28	61 Urſæ	f	7 54	n	0 46	2	F. S. R. gbM. near ½′ dia.
409	May 1	— ——	f	33 54	n	2 25	2	pB. pL. vgbM. r.
410	—	14 Canum	p	32 8	ſ	0 14	2	pB. cL. R. ſmbM. r.

II.	1785	Stars.		M.	S.		D.M.		Ob	Deſcription.
411	May 1	14 Canum	p	24	25	ſ	0	43	2	pB. pL. R. lbM. 2′ np. pBſt.
412	—	— ——	p	17	8	ſ	0	28	2	F. S. lE. glbM. er.
413	—	— ——	p	0	50	ſ	0	36	2	pB. S. R. bM. and vſE. on the edges.
414	—	— ——	f	5	58	n	0	27	1	F. S. lE.
415	—	— ——	f	48	34	n	0	15	1	F. S. iF.
416	—	— ——	f	58	10	ſ	1	8	2	pB. pL. iE. mbM.
417	—	— ——	f	58	18	ſ	0	47	1	pB. pL. iE. bM.
418	—	51 (μ) Bootis	p	69	38	ſ	1	48	1	pB. iR. mbM.
419	—	— — ——	p	68	31	ſ	0	37	1	F. pL.
420	—	— — ——	p	61	32	ſ	2	17	1	pB. vS. R. vgmbM.
421	—	— — ——	p	55	14	ſ	1	53	1	F. pL. iF.
422	—	— — ——	p	52	36	ſ	0	52	1	F. cL. iF. unequally B.
423	—	— — ——	p	47	57	ſ	0	37	1	pF. pS. iF. bM.
424	2	49 (δ) ——	p	83	12	n	0	31	1	F. pL. lbM.
425	5	34 (ν) Serpen	p	4	0	n	0	15	3	F. cS. iR. ſtellar.
426 427	Aug. 12	1 Aquarii	f	7	50	ſ	0	12	1	Two. The p. F. S. iR. mbM. The f. vF. vS. lbM. 3 or 4′ diſt. Place of 1ſt.
428	30	35 Pegaſi	f	6	22	n	0	47	2	pB. S. iR. lbM. r.
429 430	—	6 (γ) Piſcium	p	2	16	n	1	14	1	Two. The f. pB. mE. par. mbM. 4′ l. 1′ b. The p. vF. cS 3 or 4′ diſt. and p.
431	Sept. 10	92 (χ) Aqua	f	2	0	n	0	9	2	pB. S. lE. par. vgF. NM. 1′ l.
432	—	— — ——	f	22	5	n	1	9	4	pB. cL. E. 75° ſp. nf. 3′ l.
433	—	41 Ceti	p	18	0	ſ	0	4	1	pB. pL. bM. i. parallelogram. mer.
434	—	— ——	p	14	23	n	1	18	1	F. S. iF. bM. r.
435	—	67 ——	p	15	52	ſ	0	27	1	F. S. iR. bM.
436	—	— ——	f	1	45	ſ	0	14	1	F. pS. lE. ſ. 2 or 3 uneq. ſt.
437	—	— ——	f	2	7	ſ	0	24	1	F. pS. lE.
438	—	— ——	f	4	33	n	0	54	2	pB. vL. iF. mbM. r.
439	26	59 (ρ) Pegaſi	f	8	34	ſ	0	30	1	pB. pS. mbM.
440	—	— — ——	f	9	1	ſ	0	30	1	pB. pS. bM.
441	—	— — ——	f	10	1	n	0	10	1	F. S.
442	Oct. 1	62 (η) Aqua	f	9	4	ſ	0	5	3	F. S. r. lbM. or f. M.
443	—	— — ——	f	15	19	ſ	1	29	2	F. S. iR. lbM. 1′½ ſ. S. ſt.
444	—	20 Ceti	p	10	20	ſ	0	24	1	F. pL. lbM.
445	—	— ——	p	6	50	ſ	0	35	1	F. iF. er. 1′ b.
446	—	— ——	p	2	16	ſ	0	45	2	pB. S. R. mbM. m.
447	—	34 ——	f	1	3	n	2	0	2	F. S. Two more near it. See III. 592. 593.
448 449	—	43 ——	f	3	28	ſ	0	53	1	Two. Both ſtellar. within 1′ diſt. Nebuloſities run together.
450 451	3	71 (1ſt τ) Aqu	f	11	10	n	0	45	2	Two Both F. S. lE. different directions. er. 2 or 3′ from each other.
452	—	18 Ceti	p	5	33	ſ	0	59	1	pB. pS. mbM. r. ſt. 1′½ diſt.
453	5	63 (κ) Aqua	f	13	50	ſ	1	19	1	F. pL. E. par. r.

II.	1785		Stars.		M.	S.		D.M.		Ob	Deſcription.
454	Oct.	5	90 (φ) Aqua	f	3	11	n	1	17	1	F. S. almoſt ſtellar.
455		—	17 Eridani	f	11	19	n	0	26	2	Two. The p. pB. cL. E. lbM.
456					11	46		0	25		The f. eF. vS. E.
457		—	61 (ω) —	p	4	31	ſ	0	2	2	F. cL. lbM.
458		6	20 —	f	8	52	ſ	0	46	1	pB. R. bM.
459		—	— —	f	9	14	ſ	1	4	1	F. R. lbM.
460		—	— —	f	12	7	n	1	6	1	pB. S. lE. mbM. N.
461		8	111 (ξ) Piſciu	p	28	48	ſ	1	32	3	F. pL. iR. vgbM. 1′½ dia.
462		—	— — —	p	27	52	ſ	1	32	2	pB. R. vgbM. 1′¼ dia.
463		—	— — —	p	26	40	ſ	1	15	3	F. S. ilE. par. mbM.
464		—	44 Eridani	p	9	2	n	0	0	1	F. vS. r.
465		9	82 (δ) Ceti	f	7	12	ſ	0	34	3	F. pL. iR. lbM.
466		—	— — —	f	7	4	ſ	0	49	3	pB. cL. iR. mbM.
467		25	7 (b) Piſcium	p	4	23	n	1	22	1	pB. pL. iF.
468		—	26 —	f	0	11	ſ	1	10	1	F. pL. iF. r.
469		26	49 Aquarii	f	5	14	ſ	0	4	1	F. pS. lE. er. ſome of the ſt. viſible.
470	Nov.	22	67 Ceti	f	37	51	ſ	3	27	2	pB. S. ſtellar.
471		23	34 Piſcium	f	20	53	ſ	0	55	1	F. iF. lbM.
472		27	18 Ceti	f	2	18	n	1	24	1	F. pS.
473		—	47 —	f	6	3	n	0	54	1	F. S. iF. er. ſome of the ſt. viſible.
474		—	72 (ρ) —	p	9	28	n	0	56	2	pB. pL. lE. lbM.
475		—	83 (ε) —	f	24	23	ſ	0	3	1	pF. pL. iF. bM.
476		28	58 Aquarii	f	2	43	n	0	31	1	F. pL. iR. lbM.
477		—	70 —	p	2	28	ſ	0	27	1	pB. pL. iR. lbM.
478		—	17 Ceti	p	10	10	n	0	53	1	pB. L. lE. lbM.
479		—	— —	p	5	13	n	1	35	1	pB. mE. mer. 2′ l.
480		—	— —	p	2	34	n	0	34	1	F. pL. lE. lbM.
481		—	53 (χ) —	p	0	24	n	0	23	1	pB. cL. R. 1′½ f. Sſt.
482		—	55 (1ſt ζ) —	f	17	54	n	0	15	1	Four. The p. 2, both F. E. S. within 1′ diſt. par.
483											
484		—	— — —	f	17	56	n	0	11	1	The f. two, both pF. pS. E. about 2′ diſt. and nearly mer.
485											
486		—	— — —	f	20	13	n	1	5	1	F. S. E.
487		—	— — —	f	37	18	ſ	0	7	1	F. cL. iF. lbM.
488		—	— — —	f	49	13	ſ	0	50	1	F. S. iF. bM.
489		29	23 (2d θ) Arie	f	8	36	n	0	42	1	F. S. lE. contains 3 ſt. uncon.
490	Dec.	7	66 (4 σ) Canc	f	8	10	n	0	54	1	pF. mE. r. 3′ l. 1′½ b.
491		—	18 Leo. min.	p	13	13	ſ	0	30	1	pB. pL. iF. lbM.
492		—	— — —	f	1	47	n	0	0	1	pB. pL. lE. near. par.
493		—	37 — —	f	13	7	n	0	49	1	F. S.
494		—	46 Urſæ	p	3	47	ſ	0	36	1	pB. pL. iR.
495		28	3 Leonis	f	3	34	n	0	16	1	F. pL. E. iF.
496		—	9 (ο) Virgin	f	11	52	ſ	1	5	1	F.
497		—	31 (ſt d) —	p	14	27	n	1	25	1	pF. vS.

II.	1785.	Stars.		M.	S.		D.	M.	Ob	Deſcription.
498	Dec. 28	31 (1st d) Vir	p	12	30	n	1	3	1	F. pL.
499	—	— — —	p	10	55	n	1	18	1	F.
500	—	— — —	p	7	43	n	1	24	1	vL. er. ſome ſt. viſible.
501	30	52 (τ) Ceti	f	4	36	n	1	1	1	F. S. R. vSpBN.
502	—	76 (σ) ——	f	29	37	n	0	30	1	F. eS. ſtellar. p. pBſt.
503	—	— — ——	f	31	37	ſ	0	15	1	pB. S. iF. mbM.
504	—	20 Eridani	p	30	24	n	1	44	1	pB. S. lE. mbM.
505	31	9 Hydræ	f	34	16	ſ	0	15	1	pB. S. lE. ſp nf. ſmbM.
506	—	— ——	f	49	32	ſ	0	37	1	pB. S. lE. lb ſfM.
507	—	4 (ι) Crater	f	13	25	ſ	0	3	1	F. S. E.
508	—	30 (η) ——	f	4	26	ſ	0	41	1	pB. S. lE. bM.
509	—	— — ——	f	6	52	n	0	46	1	F. cL iR. lbM.
510	—	53 Virginis	f	2	58	ſ	0	25	1	F. lE. 1′½ l.
511	—	— ——	f	3	21	ſ	0	12	2	pB. pL. R. bM.
512	—	— ——	f	3	55	ſ	0	12	2	F. S.
513	—	— ——	f	4	53	ſ	0	27	2	pB. pL. iF. mbM.
	1786									
514	Jan. 1	49 Eridani	p	0	34	ſ	1	9	1	F. pL. E. ſp nf. 2′ l. 1′ b.
515	—	— ——	f	2	57	ſ	1	33	1	F. or pB. S. bM.
516	—	— ——	f	21	45	ſ	1	16	1	F. S. iR. lbM.
517	—	29 (γ) Virgin	f	19	8	n	1	22	2	pB. pL. R. bM.
518 }	2	13 Canum	p	44	34	n	2	49	2	Two. The p. F. S. E. The f. F.
519 }				44	31		2	51		S. E. in a different direction.
520	27	7 (η) Hydræ	f	24	25	n	0	7	2	F. S. lE. par. er.
521	—	77 (σ) Leonis	p	3	42	ſ	1	28	3	F. vS. iF. ſmbM. er.
522	30	47 Eridani	f	6	29	ſ	0	21	1	F. pS. iE. r. 1′ ſp. Sſt.
523	—	— ——	f	10	15	ſ	0	17	1	F. vS. iR. bM. almoſt ſtellar.
524	Feb. 1	57 (μ) ——	p	9	24	n	0	3	1	F. S. iE. lbM. p. 2 Sſt.
525	—	— — ——	p	4	5	n	1	27	1	F. pL. lE.
526	—	— — ——	f	6	16	n	0	51	1	F. cS. R. lbM.
527	—	— — ——	f	7	30	n	0	12	2	pB. S.
528	—	— — ——	f	7	40	n	0	12	1	F. S. lbM.
529	—	28 (A) Hydr	p	26	37	n	0	8	1	F. S.
530	2	60 (σ) Virg	p	52	32	n	0	19	1	F. S.
531	—	— — ——	p	47	19	ſ	1	12	2	pB. pL. E. b. ſ. M. 3′ l.
532	—	— — ——	p	35	12	ſ	1	28	2	F. pL. lbM.
533	—	64 — ——	f	26	8	ſ	1	17	2	F. pL. vlbM. 6 or 7′ l. 4′ b.
534	—	— ——	f	34	2	ſ	0	15	2	pB. vL. glbM.
535	24	10 (r) ——	f	43	43	ſ	0	39	1	F. mE. ſp ſf. 2′ l. ¾ b.
536	—	— — ——	f	48	21	ſ	0	21	1	pB. mE. mbM. 2′½ l. 1′ b.
537	—	92 ——	p	46	53	n	0	43	1	F. pL. iR. er.
538	—	108 ——	p	1	8	n	0	59	1	pB. cL iR.
539	—	110 ——	p	2	58	ſ	0	11	1	pB. cL, lE. gbM.
540	—	— ——	f	1	11	ſ	0	53	1	pB. S. mbM.
541	—	— ——	f	2	31	ſ	0	28	1	F.

II.	1786	Stars.		M.	S.		D.	M.	Ob	Deſcription.
542	Feb. 24	110 Virg	f	2	31	n	0	0	1	pB.
543	—	—— ——	f	4	14	ſ	0	34	1	F.
544	—	—— ——	f	4	52	n	0	27	2	pB. vS.
545	—	—— ——	f	6	51	ſ	1	39	4	pB. S. iE. lbM.
546	Mar. 3	6 (*b*) Leonis	p	6	16	n	1	42	1	Two. Both F. S. The place inaccurate in RA.
547										
548	—	14 Virginis	p	10	27	ſ	0	8	1	F. pL. mE. np ſf. but near. par.
549	—	26 (χ) ——	f	17	34	ſ	0	33	1	pB. vL. iF. lbM.
550	4	14 (ι) Crate	p	4	13	n	0	35	2	Two. Both F. S. lbM. cBſt. between, but 1′½ ſ. of them.
551				4	0		0	36		
552	—	21 (θ) ——	p	2	24	ſ	0	2	1	F. pS. iR. f. vSſt.
553	—	—— ——	f	11	21	ſ	1	9	2	pB. pL. iF. gbM. ſp. is 8ſt.
554	18	1 Cancri	f	4	36	ſ	0	4	2	pB. pL. er. vgmbM.
555	19	26 Hydræ	f	7	26	n	0	21	2	pB. pL. iR. b. ſ. M.
556	20	6 (3d *b*) Crat	p	76	10	ſ	1	11	3	pB. cL. iR. vgmbM.
557	24	16 (ζ) Hydræ	f	3	21	n	0	22	1	F. mE. unequally B. 3′ l. 1′ b.
558	25	21 (ψ) Virgin	f	10	43	ſ	0	38	1	F. E. mer. 3′ l. f. cBſt.
559	—	49 (*g*) ——	p	14	43	n	1	33	1	F. S.
560	—	—— ——	p	13	0	n	0	31	1	pF. pS. iR.
561	—	—— ——	p	3	39	n	0	24	1	pB. pL. R. vgmbM.
562	27	16 (κ) Crater	f	4	56	ſ	1	54	2	F. S. iR. bM. r.
563	—	68 (ι) Virgin	p	29	28	ſ	0	55	1	pB. iF. bM.
564	28	19 Urſæ	p	3	1	n	0	23	1	pB. S. R. mbM.
565	—	46 Leo. min.	p	5	3	n	0	28	1	pB. cL. iF. lbM.
566	—	*1102 (*e*) Hy	f	55	28	n	0	53	1	F. pS. E. * See note.
567	—	—— ——	f	37	17	n	0	51	1	pB. pL. iF. gbM.
568	Apr. 17	11 (*s*) Virgin	f	10	14	n	0	34	1	Four nebulæ. They are ſcattered about. The place is that of the laſt.
569										
570										
571										
572	—	—— ——	f	11	34	ſ	0	26	1	A nebula.
573	23	—— ——	f	10	18	ſ	0	26	1	A nebula, cloudy.
574	29	3 Serpentis	p	40	48	ſ	0	20	1	F. S. lE. r. p a veſt.
575	—	—— ——	p	36	3	n	0	33	1	pB. cL. iR. mbM.
576	—	—— ——	p	21	26	ſ	0	54	1	F. S. lE. like 2 ſtellar, joined cloſely.
577	30	37 Virginis	p	11	22	n	0	4	1	F. S. making a triangle with 2 Bſt.
578	—	—— ——	p	2	29	n	0	20	1	F. S.
579	—	109 ——	p	26	11	n	2	10	1	pB. cL. E.
580	—	—— ——	p	16	35	n	1	24	1	Two. The f. pB. pL. R. gbM. The n. eF. cL. diſt. 2′. The place is of f.
581										
582	—	—— ——	p	8	33	n	0	25	1	F. mE. r. 2′ l. ¼ b. f. ſt. 6m. 16″ in time.
583	May 3	14 (1ſt A) Ser	f	17	48	ſ	1	2	2	pB. S. E. nearly par. bM.
584	26	5 (*g*) Ophiuc	f	27	48	n	1	8	1	pB. cL. gbM. er. undoubtedly ſt.
585	27	3 Serpentis	p	5	43	f	1	52	1	F. S. iE. r.
586	28	40 (ξ) Ophiuc	f	28	13	n	0	57	1	pB. S. iF.

587

II.	1786	Stars.		M.	S.		D.	M.	Ob.	Deſcription.
587	June 3	61 Ophiuchi	f	0	23	n	0	36	1	F. cL. iF.
588	Sept. 4	24 (ξ) Ariet	p	39	40	ſ	0	17	2	F. S. lE. r. bM.
589	—	— — —	p	36	21	n	0	50	2	F. pL. E. b. f. M. 2′ ſp. cBſt.
590	18	2 Piſcium	f	2	2	n	0	48	1	F. S. bM.
591	—	88 (γ) Pegaſi	p	4	29	n	0	38	1	F. pL. iF. unequally B.
592	—	85 Ceti	p	3	19	n	0	5	1	pB. S. E. bM.
593	20	54 Eridani	p	61	14	n	0	43	1	pB. pS. R. reſembling I. 107. but leſs.
594	—	— —	p	55	40	n	0	10	1	pB. vS. R. bM.
595	23	66 Aquarii	p	41	2	ſ	0	1	2	F. cL. l and iE. nearly par. lbM.
596	30	51 Ceti	f	10	14	n	0	51	1	F. S. bM. 1′ ſ Sſt.
597	—	32 Eridani	p	8	30	ſ	1	10	2	F. S. E. iF. in a row with ſome ſt.
598	Oct. 13	59 (υ) Aqua	f	13	11	ſ	1	39	1	pB. pL. iR. vgmbM.
599	17	77 Cygni	f	20	15	ſ	0	6	1	F. pS. E. er.
600	—	10 Androme	f	2	5	ſ	1	14	2	pB. mE. np ſf. but near. mer. lbM. r. 5′ l. 1′½ b. alſo ob. 1784.
601	—	26 (β) Perſei	p	15	16	n	1	14	1	F. S. iF. r.
602	—	— — —	p	13	38	n	0	34	1	F. pS. iR. lbM.
603	—	— — —	f	11	27	n	0	35	1	pB. ſtellar. or pcſt. with S. vF. chev.
604	18	59 Androme	p	2	10	ſ	0	17	1	pB. cL. lE. mbM.
605	—	— —	p	0	54	n	0	9	1	pB. S. iF.
606	24	6 Lacertæ	p	17	44	n	2	18	3	F. S. er. or rather a patch of ſt.
607	—	30 Perſei	p	12	50	ſ	1	44	1	F. cL. E.
608	—	— —	p	11	45	n	0	19	1	F. cL. er. ſome ſt. viſible.
609	26	65 (ι) Piſcium	p	1	55	ſ	0	6	1	pB. S. iR. gbM.
610	—	90 (υ) —	f	24	26	n	1	31	1	F. S. bM. r.
611	—	— — —	f	27	38	n	0	41	1	F. S. lE.
612	—	10 (α) Triang	p	28	30	ſ	1	8	1	pB. pL. lE. nearly par. mbM.
613	—	— — —	p	4	46	ſ	0	47	1	F. S. lE. par. bM.
614 615	—	34 (θ) Gemin	p	5	37	ſ	0	25	1	Two. The ſ. F. S. R. bM. The n. F. cS. R. bM.
616	—	66 (α) —	f	9	32	ſ	0	11	1	F. S. lbM.
617	Nov. 13	6 (β) Arietis	p	3	55	n	0	56	1	F. cL. vglbM.
618	—	— — —	p	3	23	n	1	45	1	vS. ſtellar.
619	—	52 —	p	5	39	ſ	0	3	1	pB. cL. pmE. mer. r. 1′ ſ. ſt.
620	Dec. 11	27 (κ) Perſei	p	5	48	n	1	31	2	F. S. iR. bM. L. ſtellar.
621	13	34 Ceti	p	23	45	ſ	0	34	1	F. E. np ſf. lbM. 1′½ l.
622	20	26 —	f	9	8	ſ	0	22	1	F. R. bM. er.
623	21	2 (ε) Corvi	p	16	4	ſ	0	33	2	F. S. E. mer. or few deg. np ſf. lb. ſ. M.
624	29	1 Sextantis	f	8	54	ſ	1	8	1	F. lE. nearly par. 1′½ l.
625	—	29 (γ) Virgin	p	17	56	ſ	1	58	2	pB. mE. 20° ſp nf. 2′ l.
626	30	77 (σ) Leonis	p	4	44	ſ	1	30	1	pB. S. lE. mbM.
	1787									
627	Jan. 11	55 (δ) Gemi	f	54	51	ſ	0	26	3	F. S. iF. lE. ſp nf.
628	14	6 Comæ	f	6	36	n	0	38	1	pB. cL. E.
629	—	— —	f	13	46	ſ	0	49	1	F.

II.	1787	Stars.		M.	S.		D.	M.	Ob	Deſcription.
630	Jan. 14	6 Comæ	f	13	20	f	0	56	1	cL.
631	—	— —	f	16	3	f	1	31	1	F.
632	—	29 —	p	8	57	n	1	12	1	F. pL. R. vgbM.
633	17	16(1ſt p)Perſ	p	7	2	f	1	1	1	F. cL. lbM. 4′ dia.
634	Feb. 13	33 (*) Cancri	p	12	7	n	0	34	1	F. S. bM.
635	22	21 (θ) Crater	p	13	5	n	1	9	1	F. pS. iR. vgbM.
636	—	65 Virgin	p	43	8	f	0	49	1	F. vL. bM.
637	March 11	44 (k) —	f	12	41	f	0	36	1	F. cL. iR. lbM. time inaccurate.
638	15	*1139(r)Ce	f	22	49	f	0	12	1	pB. S. lE. ſp nf. * See note.
639	17	32 Leo. min.	p	16	31	f	0	11	1	pB. cS. r.
640	—	— — —	p	16	11	f	0	18	1	F. vS. r. with 300 the ſame.
641	—	38 —	f	2	41	f	0	36	2	F. vS.
642	—	6 Canum	p	15	18	n	0	30	2	pB. S. E.
643	—	10 —	p	0	37	f	2	11	1	F. pL. gbM. r.
644	—	— —	f	2	55	f	1	1	1	pB. S. R. mbM. among ſcattered ſt.
645	—	— —	f	4	33	f	1	2	1	pB. S. R. mbM.
646	—	17 —	f	12	21	n	0	12	1	pB. L. iF. uneq. B. 3. or 4′ dia.
647	—	12(λ)Coronæ	f	33	4	n	1	27	1	F. S. iF.
648	18	53(2d,)Boot	p	55	31	n	1	11	2	pB. pL. lbM.
649	—	— — —	p	54	11	f	0	13	2	F. S. E. nearly mer. r.
650	—	— — —	p	16	19	n	1	13	3	pB. E. BNM. and F. br. 2′l. ¼ b.
651	—	— — —	p	5	42	n	0	51	2	pB. pL. iE. er.
652	—	30 (g) Hercul	p	0	57	f	0	57	1	F. pL. r.
653	19	70 Virginis	p	4	21	n	0	11	1	pB. vS. mbM. juſt p. pcſt.
654	—	9 Serpentis	f	7	56	f	0	28	1	F. E. np ſf. 1′½ l.
655	—	— —	f	15	44	n	0	16	1	F. E. mer. 1′½ l.
656	—	— —	f	16	59	f	1	17	1	pB. E. np ſf. bM. 1′½ l.
657	—	28 (β) —	f	8	2	f	0	52	1	F. iF. bM. 1′¼ dia. between 2Bſt.
658	20	44 Lyncis	p	47	39	f	0	23	1	pF. vS. mbM.
659	—	13 Canum	p	18	44	n	1	47	1	F. S. R. juſt np. V. 42.
660	April 9	8 —	f	7	58	f	0	5	1	pB. pL. R. mbM.
661	—	— —	f	9	42	f	0	20	1	pB. vS. ſtellar. juſt p. Sſt.
662	—	— —	f	15	2	n	0	36	1	F. S. R. bM.
663	—	19 —	p	9	58	n	0	56	1	pB. vS. ſtellar. near and n. Sſt.
664	—	— —	p	3	47	n	3	13	2	pB. mE. ſp nf. near. mer. 5′ l. ¾ b.
665	—	20 —	f	2	52	n	2	31	1	pB. cS. E. with 300 ſt. with burrs.
666	—	— —	f	5	24	n	2	30	1	pB. S. iR. mbM.
667	—	— —	f	7	35	n	2	42	1	pB. vS. lE. bM.
668	—	— —	f	27	51	n	0	51	1	F. E. par. miniature of I. 170.
669	—	— —	f	33	20	n	0	41	1	pB. pL. vgmbM.
670	—	— —	f	35	10	n	2	37	1	pB. pL.
671	—	— —	f	37	51	n	0	35	1	pB. pL. E.
672	—	— —	f	43	59	n	0	17	1	pF. pS. bM.
673	—	— —	f	66	36	n	1	0	1	F. pL. E. vlbM.
674	—	— —	f	71	16	n	0	27	1	pB. E. nearly par. 1′½ l. ½ b.

II.	1787	Stars.		M.	S.		D.	M.	Cl.	Deſcription.
675	April 9	20 Canum	f	80	7	n	0	51	1	F. vS.
676	—	— ——	f	98	12	n	1	42	1	pB. vS. ſtellar.
677	—	— ——	f	99	9	n	1	39	1	F. pS. lbM.
678	—	— ——	f	117	42	n	1	1	1	F. S. r. in a row with 3 ſt.
679	11	79 (ζ) Virgin	p	4	17	f	1	1	2	Two. The p. F. pS. iF.
680				4	7		1	4		The f. pB. pL. iF. bM.
681	—	1 Serpentis	p	19	44	f	0	7	2	pB. pL. iF.
682	—	— ——	p	16	35	f	0	4	2	pB. cS. lE.
683	—	— ——	f	0	49	f	0	55	1	pB. pL. R. mbM. ſſ. cſt.
684	—	4 ——	p	6	6	n	0	7	1	Two. The 2d pB. S. iE. for the 1ſt ſee II. 545.
685	15	90 (ρ) Virgin	p	2	37	f	0	44	2	F. pL. iR. f. and par. with 2Fſt.
686	—	— — ——	p	0	37	n	0	4	2	pB. S mbM.
687	—	102(1ſt υ) —	p	6	18	f	0	57	2	pB. cL. mE. 20° ſp nf.
688	May 11	19 (λ) Bootis	p	30	37	n	0	7	2	F. mE. 15° ſp nf. lbM. 4′l. ¾ b.
689	12	— — ——	p	47	20	n	0	46	3	pB. pL. R. mbM.
690	—	22 (τ) Hercu	f	7	2	n	2	3	2	F. pL. iF. gbM.
691	15	85 (η) Urſæ	f	15	34	f	0	12	1	pB. pL. E. nearly par. mbM.
692	—	— — ——	f	19	36	n	1	20	1	Two. The p. F. pS R. vgbM. The f. F. vS. ſtellar. ſmbM. diſt. 2′½.
693										
694	—	24 (g) Bootis	p	6	31	n	0	43	1	pF. pS. lE. mbM.
695	—	— — ——	f	1	7	f	0	12	1	pB. cL. iR. vgmbM.
696	—	— — ——	f	3	40	n	0	3	1	pB. S. E.
697	16	*C Canu. 6m	f	6	23	f	0	39	1	F. E. par. bM. 1′½ l. 1′ b. *See note.
698	—	— —— —	f	10	0	f	0	58	1	F. S. R. vſmbM.
699	—	— —— —	f	13	19	n	0	20	3	F. pL. R. lbM. 1′½ dia.
700	—	27 (γ) Bootis	f	5	15	n	0	9	1	pF. S. iE.
701	—	25 Herculis	f	17	43	f	0	40	1	pB. pS. E. ſp nf. vgmbM.
702	Sept. 11	68 (2d g) Aqu	f	4	23	f	1	1	1	pF. pL. E. np ſf. but near. par. mbM. 1′½ l.
703	—	*A Ceti 7m	f	4	47	n	1	7	1	F. cL. E. * See note.
704	16	47 Caſſiop	f	61	37	n	3	48	2	F. pL. mE. np ſf. mbM.
705	Nov. 3	25 Cephei	f	21	6	f	1	35	1	pB. S. iR. or. almoſt equally B.
706	—	1 (e) Caſſiop	f	6	26	n	2	5	2	pBM. 2ſt. involved in nebuloſity. 2′ l. 1′½ b.
707	30	19 (ξ) ——	p	2	50	f	2	12	1	pB. vL. iR. vgmbM. r. 5 or 6 dia.
708	1788 Jan. 14	37 Lyncis	f	3	50	f	1	15	1	pB. S. ſtellar.
709	—	56 Urſæ	p	6	51	f	1	57	2	pB. S. lE. mer. bM.
710	—	27 (γ) Bootis	p	43	42	n	1	51	1	F. S.
711	—	— — ——	p	42	47	n	1	50	1	pB. cL. iF.
712	—	— — ——	p	41	48	n	1	25	1	F. S. R. bM.
713	—	— — ——	p	39	13	n	2	12	2	pB. pL.
714	—	— — ——	p	39	5	n	2	9	2	Two. Both pB. S. R. 2′ diſt. in the ſame mer.
715										

II.	1788	Stars.		M.	S.		D.	M.	Ob	Deſcription.
716	Jan. 14	27 (γ) Bootis	p	36	48	n	2	19	2	pB. L. iR: FN. mbM. 4 or 5′ dia.
717	15	15 Leo. min.	f	0	58	ſ	1	58	1	F. pL. iF. lbM.
718	—	45 (ω) Urſæ	p	2	24	n	0	32	2	pB. S. lE. the np. corner of a trapezium.
719	Feb. 3	32 Lyncis	p	20	34	ſ	0	16	1	F. pL. iR. bM.
720	—	34 (μ) Urſæ	p	2	13	n	1	29	1	F. vS.
721	—	— — ——	p	1	57	n	1	26	1	F. vS. ſtellar.
722	—	— — ——	p	1	43	n	1	27	1	F. vS. ſtellar.
723	—	13 Canum	p	73	0	ſ	0	22	1	pB. S. lE.
724	—	— ——	p	65	22	ſ	0	44	1	F. vS.
725	—	— ——	p	61	59	ſ	0	19	1	pB.E.ſp nf.but nearer mer.mbM.2′l
726	5	80 (π) Gemi	f	22	56	n	0	38	1	pF. pL. iR. lbM. r. ſ. 2 ſt. par.
727	—	59 (2 σ) Canc	p	13	13	n	1	47	1	pF. pL. iR. r.
728	—	60 Urſæ	p	25	2	n	1	32	2	pB. pL. vgmbM.
729	—	— ——	p	20	38	ſ	1	4	2	F. cL. lE. par. lbM.
730	—	— ——	p	5	27	n	0	14	1	pB. bM. r. 4′ l. 3′ b.
731	—	— ——	p	0	54	n	1	5	2	pB. S. E. ſp nf.
732	—	— ——	f	0	47	ſ	0	19	1	F. S. almoſt. betw. 2ſp. ſt. chev. touches them.
733	6	59 ——	f	20	28	n	0	21	1	pB.mE.mer.pBSN.&vF.br.4′l.¾′b.
734	9	20 Lyncis	p	14	20	n	0	28	1	F.pL.iF.mbM.ſf. a triangle of 3.ſt.
735	—	— ——	p	12	44	ſ	1	37	1	F. ſtellar.
736	—	— ——	p	11	9	ſ	0	3	1	pF. vS. lbM. r.
737	—	63 (χ) Urſæ	p	4	55	ſ	0	4	2	pF. pS. iR. lbM.
738	—	— — ——	f	2	30	n	0	57	1	pB. pL. R. mbM.
739	—	— — ——	f	2	50	n	0	56	1	F. vS.
740	—	— — ——	f	5	48	n	0	56	1	pF. pS. ſtellar.
741	—	— — ——	f	17	7	n	0	53	1	pF. S. R. gbM.
742	—	3 Canum	p	1	50	ſ	1	35	2	F. S. E.
743	—	— ——	f	5	22	ſ	0	10	1	F. S.
744	—	— ——	f	21	50	n	1	27	2	pF. S. er.
745	April 1	Neb. II. 728.	p	35	22	ſ	0	57	2	pF.pS.E.ſ.&ſp.ſt.among ſt.not con.
746	8	54 Virginis	p	0	24	ſ	0	43	1	pB. S. pBN.
747	10	60 Urſæ	f	31	58	ſ	0	24	1	pB. E. 15 or 20° np ſf. 3′ l.
748	—	— ——	f	38	3	n	0	16	1	pB. pL. E. ſp. and in a line with 2ſt.
749	—	— ——	f	47	57	ſ	1	10	2	pB. pL. iF.
750	27	19 (λ) Bootis	p	109	46	ſ	1	1	1	pF. pL. E. ſp nf.
751	—	37 (ξ) ——	f	16	12	n	0	25	2	Two. The p. cF. cS. The f. pF. pL.
752				16	20		0	24		Both lE. np ſf. but nearer par.
753	28	27 (β) Hercul	f	2	50	ſ	1	42	1	pF. pS. vlE. mbM.
754	29	27 (γ) Bootis	p	11	15	n	1	27	1	pB. pL. R. FN.
755	May 1	23 (θ) ——	f	44	59	n	0	31	1	pB. pL lE.
756	5	Neb. II. 757.	p	11	47	ſ	3	7	2	pB. pL. iF. r.
757	—	12 (ι) Draco	p	16	38	ſ	1	56	3	pB. S. iR. or lE. mbM.
758	—	Neb. II. 757.	f	5	28	ſ	1	31	1	pF. pS. iR.

II.	1788	Stars.		M.	S.		D.	M.	Ob	Deſcription.
759	May 5	Neb. II. 757.	f	6	6	ſ	0	42	1	pB. FNM. 8 or 10′ l. 2′ b.
760	—	— — —	f	6	29	ſ	1	37	1	pF. pS. R.
761	—	— — —	f	24	8	ſ	0	33	1	pF. pS. iF.
762	—	— — —	f	24	37	ſ	0	25	1	pF. pL. E.
763	25	12 (ι) Draco	p	13	7	n	0	54	2	pB. mE. nearly mer. 2′ l. ½′ b.
764	—	— — —	f	13	58	n	0	20	1	pB. S. iR. one p. ſuſpected vF. lE.
765	—	— — —	f	14	36	ſ	0	58	1	pF. cS.
766	—	— — —	f	15	0	n	0	18	1	pB. cL. iE. r.
767	June 6	31 (1ſt ψ) —	p	31	23	n	0	15	1	pB. pL. R. vgmbM.
768	Nov. 4	14 Camelop	p	42	52	n	1	57	1	pB. S. lE. BN. juſt f. pB. ſt.

Third claſs. Very faint nebulæ.

III.	1785	Stars.		M.	S.		D.	M.	Ob	Deſcription.
377 378	April 26	92 Leonis	f	3	6	ſ	1	24	2	Two. The n. F. S. lbM. The f. vF. vS. diſt. 5′ ſp. the place of n.
379	—	1 Comæ	p	10	26	ſ	0	6	3	vF. vS. lE. er. or S. patch of ſt.
380	—	— —	p	8	56	ſ	1	7	2	F. S.
381	—	— —	p	7	56	ſ	1	12	1	vF. R.
382 383 384	—	2 —	f	1	58	ſ	0	49	2	Three. The place is of the laſt or moſt n. which is vF. S. The other two are ſp. eF. vS.
385	27	93 Leonis	p	2	54	ſ	0	35	1	vF. vS. r.
386	—	— —	p	3	10	ſ	0	27	1	vF. vS. r.
387	—	— —	p	2	10	ſ	0	27	1	vF. vS. r.
388	—	— —	p	1	48	n	0	11	2	vF. pL. iR. lbM. r. 7′ nf. cBſt.
389	—	— —	f	4	49	n	0	25	1	vF. vS.
390	—	5 Comæ	p	8	56	ſ	1	47	1	Suſpected.
391			p	7	54	ſ	0	9	2	Six nebulæ. The places belong to the three firſt which are vF. vS. The other three are 10 or 12′ more ſouth, but there was not time to take their places. more ſuſpected.
392			p	7	56	ſ	0	13	2	
393	—	— —	p	7	54	ſ	0	15	2	
394										
395										
396										
397	—	— —	f	3	36	n	0	5	2	vF. vL. iR. bM. 6′ l. 5′ b.
398	—	26 —	f	8	12	ſ	1	33	1	vF. vS. r.
399	28	61 Urſæ	f	31	26	n	1	57	2	vF. pL. lE. r.
400	May 1	— —	f	25	34	n	2	38	4	vF. vS. ſtellar. 2′½ n. Sſt.
401	—	14 Canum	f	2	36	ſ	0	36	1	vF. ſtellar. with 300 the ſame.
402 403	—	— —	f	19	23	n	0	35	1	Two. Both vF. cS. The place is that of the p. The 2d, 3′ nf.
404 405	—	— —	f	20	44	n	0	7	1	Two. Both vF. pS. The place is of the p. The 2d, 5 or 6′ nf.
406	—	— —	f	25	14	ſ	0	59	1	vF. vS. lE.

III.	1785	Stars.		M.	S.		D.	M.	Ob	Deſcription.
407	May 1	49 (δ) Bootis	p	102	40	n	1	37	2	Two. Both vF. vS. A ſtar between them about half way.
408				102	22		1	39		
409	—	14 Canum	f	30	38	f	0	17	1	vF. pL. R. lbM.
410	—	— —	f	35	6	n	1	1	1	vF. S. lE. er.
411	—	— —	f	54	30	f	1	8	1	eF. vS.
412	—	— —	f	54	45	n	0	25	1	vF. vS.
413	—	— —	f	58	30	f	0	53	1	vF.
414	—	51 (μ) Bootis	p	70	6	f	0	55	1	vF. mE.
415	—	— — —	p	65	4	f	1	57	1	eF. pL.
416	—	— — —	p	64	2	f	1	57	1	Two. Both vF. S. diſt. 6 or 7′. The place is that of the ſf.
417										
418	—	— — —	p	62	52	f	0	6	1	eF. ſtellar.
419	—	— — —	p	61	4	f	0	42	1	vF. vS. E. er.
420	—	— — —	p	54	54	f	0	50	1	vF. S.
421	—	— — —	p	49	26	f	0	40	1	vF. vS.
422	2	49 (δ) —	p	86	2	n	0	24	1	Two. Both eF. ſtellar. diſt. 4 or 5′. nearly mer. The n. fainteſt.
423										
424	3	— — —	p	147	32	n	0	11	1	vF. ſtellar. or little larger.
425	—	— — —	p	101	48	n	1	39	1	vF. vS. in the field with III. 407. 408.
426	Aug. 30	17 (ι) Piſcium	p	8	48	f	1	42	1	eF. pL. iR.
427	—	19 —	f	0	14	n	0	19	1	vF. S. lE. nearly mer.
428	Sept. 10	30 —	f	14	30	f	0	19	2	vF. S. iF. lbM.
429	—	41 Ceti	p	26	42	n	0	35	1	vF. pS. E.
430	—	— —	p	26	54	n	0	44	1	vF. vS.
431	—	Neb. I. 100.	f	0	22	n	0	0	1	The 2d of two. eF. S. 5 or 6′ diſt. from I. 100.
432	—	41 Ceti	f	15	36	n	0	22	1	eF.
433	—	67 —	p	15	39	n	0	59	1	vF. vS.
434	—	— —	f	18	40	f	0	42	1	vF. cL. iF. lbM. 4 or 5′ l. 2 or 3′ b.
435	26	59 (ρ) Pegaſi	f	8	42	f	0	20	1	vF. vS.
436	—	32 (2d c) Piſc	f.	1	20	f	1	1	1	vF. pL. lbM.
437	27	26 —	p	7	39	f	0	13	1	eF. vS. er. confirmed by 240.
438	28	93 (2 ψ) Aqua	f.	9	22	f	0	15	1	eF. S. ſtellar. p. 1′½. pBſt.
439	Oct. 1	20 Ceti	p	0	42	f	1	3	2	vF. S. iE.
440	—	38 —	f	1	5	n	0	8	1	vF. vL. requires great attention.
441	—	43 —	f.	5	8	f	1	29	1	vF. vS. iE.
442	—	— —	f	5	23	f	1	26	1	vF. vS. iE.
443	5	17 Eridani	p	17	51	f	0	13	1	vF. vS. confirmed by 240.
444	—	— —	p.	9	23	n	0	25	1	eF. vS.
445	—	— —	p	5	37	f	0	41	1	vF. pS. E.
446	—	— —	f	3	4	f	0	3	1	vF. S. between ſome Sſt.
447	—	20 (τ) Orion	f	10	23	n	1	32	2	vF. cL. iR. near a hook of vSſt.
448	—	— — —	f	34	45	f	0	26	3	vF. S. R. r. lbM.
449	6	1 (1ſt τ) Erid	f	4	8	n	1	34	1	vF. pL. broadly E. lbM.
450	—	— — —	f	6	30	n	1	56	2	vF. S. lE.

III.	1785		Stars.		M.	S.		D.	M.	Ob	Deſcription.
451	Oct.	6	20 Eridani	f	2	30	ſ	0	59	1	vF. S. R.
452		8	52 (π) Aqua	p	30	46	n	1	39	1	vF. pL. R. r.
453		—	10 Orionis	f	5	7	ſ	0	4	1	vF. vS. confirmed 240.
454		9	60 Ceti	p	27	18	n	0	27	1	eF. pL. 240. left doubtful.
455		—	82 (δ) ——	f	4	11	n	1	2	2	vF. vL. lbM. er. 6 or 7′ dia.
456		25	28(ω)Piſcium	f	13	6	ſ	0	28	1	vF. pS. iF.
457		—	78 (ν) Ceti	p	20	29	n	0	20	1	vF. cL. vlbM. m. p. Bſt. and joining.
458		26	49 Aquarii	p	2	52	n	0	6	1	vF. S. er. time inaccurate.
459		—	56(1ſt υ) Ceti	p	7	44	ſ	1	17	1	vF. vS. er.
460		—	—— ——	p	2	55	ſ	1	16	1	vF. vS.
461		27	18(ι) Piſ. Au.	f	90	20	n	1	56	1	vF. cL. lE. glbM. 4 or 5′ l.
462	Nov.	7	82 (δ) Ceti	f	8	1	ſ	0	36	1	vF. S.
463		22	25 ——	p	12	56	ſ	0	23	2	vF. pL. iR. r.
464		—	67 ——	p	20	11	n	0	59	1	eF. S. found in gaging.
465		23	46 (ξ) Pegaſi	f	11	21	n	0	54	1	eF. S. iF. 240 the ſame.
466		—	82 ——	f	5	54	ſ	0	15	1	vF. S. R. lbM.
467		27	18 Ceti	p	11	15	n	0	12	1	eF. vS. 240 left ſome doubt.
468		—	72 (ρ) ——	p	27	13	n	0	43	1	vF. E. nearly mer. lbM. 1′½ l. 1′b.
469		—	83 (ι) ——	f	19	27	ſ	0	30	1	vF. ſtellar. 240 left ſome doubt.
470		28	91 Aquarii	p	1	53	ſ	0	7	1	eF. vS. 240 left doubtful.
471		—	53 (χ) Ceti	p	13	54	n	0	40	1	A few Sſt. mixed with nebuloſity.
472		—	55(1ſt ζ)——	f	41	48	ſ	0	18	1	vF. pL. vlbM. near ſcattered ſt.
473		29	87 (υ) Pegaſi	p	44	53	ſ	1	26	1	eF. cL. ſome doubt. p. a row of ſt.
474		—	23(2d θ) Arie	f	7	29	n	0	50	1	eF. vS. iR. confir. 240.
475		—	34 (μ) ——	p	1	44	ſ	0	44	1	vF. S. confir. 240.
476	Dec.	5	34 (ζ) Andro	p	11	14	ſ	0	23	1	vF. vS. ſtellar. ſp. pBſt.
477		—	36 ——	p	2	25	n	0	44	1	vF. S. R. juſt p. vFſt.
478		7	20 Leo. min.	f	1	20	n	0	47	1	eF. S. left doubtful.
479		26	2(ι)Can. min	f	26	18	n	0	25	1	ſuſpected. eF. vS. lE.
480		28	9 (o) Virgin	f	12	46	ſ	2	5	1	vF. L. ſeen by looking at II. 137.
481		—	31(1ſt d)——	p	17	49	n	1	44	1	vF.
482		—	—— ——	p	15	22	n	1	39	1	eF.
483		—	—— ——	p	12	49	n	1	24	1	vF.
484		—	—— ——	p	11	8	n	1	34	1	vF.
485		30	46 Ceti	p	40	9	ſ	1	4	2	vF. S. iF. r.
486		—	76 (σ) ——	p	12	32	ſ	0	52	1	vF. vS. iF. better with 240.
487		—	20 Eridani	p	3	52	n	2	14	1	vF. S. E.
488		31	9 Hydræ	f	38	13	ſ	0	26	1	vF. cL. gvlbM. 3′ l. 2′ b. p. pBſt.
489		—	53 Virginis	p	18	36	ſ	0	47	1	vF. S. lbM.
	1786										
490	Jan.	1	45 Eridani	p	11	41	ſ	0	42	1	vF. vS. lE. better with 240.
491		—	13 (π) Virgin	p	16	10	n	0	35	2	vF. S. R. bM.
492		—	15 (η) ——	f	7	0	ſ	0	15	2	vF. cL. mE. r.
493		—	29 (γ) ——	p	6	35	n	1	12	2	eF. S. iF.
494		—	—— ——	f	1	24	n	0	48	2	vF. pS. E.

III.	1786		Stars.		M.	S.		D.M.		Ob	Deſcription.
495	Jan.	2	61 Urſæ	f	58	0	ſ	0	46	1	eF. S. iF. r.
496		—	— ——	f	70	52	ſ	0	3	1	eF. vS. pmE.
497		27	36 Sextantis	f	6	47	n	1	20	2	cF. S. R. vlbM.
498		—	58 (d) Leonis	f	0	43	n	0	1	1	vF. mE.
499		30	39 (A) Erida	p	6	26	n	1	25	1	vF. S. E. er.
500		—	69 (λ) ——	p	3	50	ſ	0	24	1	cF. S. iF. bM.
501	Feb.	1	57 (μ) ——	f	4	13	n	0	30	1	vF. vS.
502		—	— — ——	f	6	2	n	0	39	1	vF. S.
503		—	— — ——	f	14	49	ſ	0	1	1	vF. pL. ſp. 2pBſt. equil. triang.
504		2	60 (σ) Virg	p	38	27	n	0	34	2	vF. pS.
505		—	64 ——	f	16	1	ſ	0	39	2	vF. vS. R.
506		—	— ——	f	32	47	n	0	7	1	vF. E. 2′ l.
507		4	82 ——	p	9	23	ſ	0	4	1	vF. vS. er. 240 rather confir.
508		—	19 Libræ	p	18	52	ſ	0	27	1	vF. cL. iE. nearly mer.
509		22	5 (β) Virgin	f	49	54	ſ	0	35	1	vF. vS.
510		24	55 Orionis	f	1	13	n	0	7	1	eF. E. er. probably a patch of ſt.
511		—	110 Virginis	f	3	5	ſ	0	25	1	vF. R. precedes I. 128. 7′½. and is 5′ n
512	March	3	17 (β) Cancri	p	14	9	n	0	9	1	vF. S. R. mbM. 240 ditto.
513		—	6 (b) Leonis	f	2	1	n	0	25	1	eF. vS. ſtellar. 240 verif.
514		—	26 (χ) Virgin	f	10	4	ſ	1	8	2	eF. S. mE.
515		—	— — ——	f	12	19	ſ	0	26	1	vF. S. E.
516		—	— — ——	f	14	18	ſ	0	41	1	vF. S.
517		—	— — ——	f	14	43	ſ	0	48	1	vF. S.
518		19	41 (λ) Hydræ	p	0	28	ſ	0	5	1	vF. S. R. in the field with λ
519		24	1 Sextantis	f	1	47	n	0	7	1	vF. pL. vgvlbM. betw. 2 groups of ſt. np. ſf.
520		25	27 Hydræ	f	3	9	ſ	0	51	1	vF. S. E.
521		—	— ——	f	22	39	ſ	0	45	1	cF. pS. lE.
522		—	14 (ι) Crater	p	34	1	ſ	2	2	2	cF. pL. iR. lb. near M.
523		—	21 (q) Virgin	f	13	23	ſ	0	38	1	vF. E. ſp nf. 4′ l. 3′ b.
524		—	— — ——	f	15	14	ſ	1	59	2	cF. mE. r. 4′ l. ¾′ b.
525		—	49 (g) ——	p	14	19	n	1	14	1	vF. vS.
526		—	— — ——	p	13	15	n	0	8	1	eF. eS. ſome little doubt.
527		27	8 Sextantis	p	10	33	ſ	0	31	3	vF. S. ~~iR. vgbM~~.
528		—	— ——	p	9	10	ſ	1	32	1	vF. S. E. nearly mer.
529		—	16 (κ) Crater	p	13	0	ſ	1	46	1	eF. S.
530		—	— — ——	p	3	32	ſ	1	30	1	vF. ſtellar.
531		—	— — ——	p	2	47	ſ	1	32	1	cF. ſtellar. vlbM.
532		—	— — ——	f	1	7	ſ	0	51	1	vF. lE. vlb. about M.
533		—	24 (ι) ——	f	28	31	ſ	0	59	1	vF. S. iF. time a little inacc.
534		—	— — ——	f	33	51	ſ	0	48	1	vF. pL. of uneq. light.
535		—	— — ——	f	40	50	n	0	58	2	vF. pS. iF.
536		—	68 (ι) Virgin	p	36	17	ſ	0	33	1	cF. ſtellar.
537		—	— — ——	p	34	23	ſ	0	39	1	vF. vS. iF.
538		—	— — ——	p	31	24	n	0	8	2	eF. S. er.

III.	1786	Stars.		M.	S.		D.	M.	Ob	Deſcription.
539	Mar. 27	68 (ι) Virgin	p	5	57	ſ	1	21	1	vF. vS.
540	28	19 Urſæ	p	15	27	n	1	3	2	vF. S. E. 20° np ſf. contains 2vFſt.
541	—	8 Leo. min.	f	9	41	n	0	50	3	cF. S. iR. gbM. r. 1′½ dia.
542	—	21 — —	p	7	55	n	0	8	3	cF. vL. iF. 5′ l. 4′ b. ſp. a double ſt.
543	April 17	11 (ſ) Virgin	f	37	39	ſ	1	31	1	eF. pL.
544	—	— — —	f	43	12	ſ	1	23	1	vF. vS.
545	—	— — —	f	62	44	ſ	1	9	1	eF. cS. er.
546 547	29	64 —	f	36	17	n	1	4	1	Two. Both vF. vS. r. the place betw. them. ſp nf. but near. mer.
548	30	43 (δ) —	p	0	31	ſ	0	30	1	vF. cS. with 240 lE. near vSſt.
549	—	84 (ο) —	f	9	1	ſ	1	13	1	eF. vS. ſtellar. confir. 240.
550	—	109 —	p	5	32	n	1	35	1	vF. S. p. and in a line with 2Bſt.
551 552	May 1	31 Bootis	p	23	9	ſ	0	42	1	Two. Both eF. vS. The place is that of the f. diſt. 3 or 4′
553	3	50 (σ) Serpen	p	12	7	ſ	0	15	1	cF. iF. r. 5′ l. 3′ b.
554	27	3 —	p	21	20	ſ	1	19	1	vF. S. E. np ſf. but nearly mer.
555	June 22	101 Hercul	p	2	55	ſ	1	28	1	cF. S. lE. iF. r.
556	Sept. 4	71 (ε) Piſcium	f	22	10	n	1	24	1	vF. mE. 75° ſp nf. 1′½ l.
557	18	85 Ceti	p	6	18	n	0	52	1	vF. vS. lE. r. 240 the ſame.
558	20	97 Aquarii	p	14	9	ſ	0	33	1	eF. cL. iR. 5 or 6′ dia.
559	—	54 Eridani	p	65	18	ſ	0	59	1	3vSſt. in a line with vF. nebuloſity.
560	21	45 Androm	f	16	14	ſ	0	32	1	vF. S. E. among ſt.
561	—	58 —	p	17	32	ſ	1	34	1	vF. ſtellar.
562 563 564 565	—	— —	p	15	22	ſ	1	48	1	Four. ſtellar. unequal. Three in a row, and the fourth making a a rectangle with them. That at the angle is much larger.
566	—	— —	p	5	4	n	0	14	2	vF. pL. iR.
567	—	— —	f	2	29	ſ	0	12	1	vF. S. lE.
568	30	17 Eridani	p	8	17	n	2	17	1	eF. S. iF. among 3 or 4 ſt.
569	—	— —	f	9	13	n	0	27	1	eF. lE. er.
570	Oct. 17	26 (β) Perſei	p	43	39	n	0	51	1	eF. vS. lE.
571	—	— — —	p	42	9	n	0	44	1	eF. ſtellar. not verified.
572 573	—	— — —	p	32	26	ſ	0	11	1	Two. Both vF. vS. er. diſt. 4′. the place between them.
574 575	—	— — —	f	13	6	n	0	27	1	Two. Both vF. ſtellar. vlbM. but the ſ. is the brighteſt and largeſt.
576	18	12 Androm	p	24	27	ſ	1	47	1	vF. S. iR. ſtellar.
577	—	53 (τ) —	p	18	55	ſ	0	8	1	vF. pL. lE. lbM.
578	—	28 (ω) Perſei	p	2	50	ſ	1	16	1	vF. vS.
579	24	17 (ι) Andro	p	3	21	n	1	3	1	vF. vS. juſt f. pBſt.
580	—	30 Perſei	p	20	43	ſ	1	3	1	ſuſpected. r. ſome ſt. viſible.
581	25	40 Arietis	p	8	24	ſ	0	18	1	vF. E. iF. time inaccurate.
582	—	— —	p	1	17	ſ	2	7	1	vF. S. iF.
583	26	10 (α) Triang	p	18	21	ſ	0	29	1	vF. vS. E. or 3Fſt. with vF. Nebul.

III.	1786	Stars.		M.	S.		D.	M.	Ob	Deſcription.
584	Oct. 26	35 Arietis	p	0	41	n	0	50	1	vF. S. bM.
585	Nov. 26	48 (ν) Eridani	p	3	33	ſ	1	2	1	ſuſpected; hazy weather.
586	—	— — —	p	3	6	ſ	0	56	2	eF. S. E. nearly par. another ſuſpec. 3′ ſf. ſtellar.
587	28	42 (ξ) ——	f	2	34	n	0	9	1	vF. S. bM. betw. 2 ſt.
588	—	— — ——	f	7	35	ſ	1	57	1	vF. S.
589	—	— — ——	f	10	16	ſ	1	22	1	vF. cL. iE. nearly par. bM.
590	Dec. 14	8 Leporis	f	9	18	ſ	0	6	1	eF. ſtellar. a little doubtful.
591	15	13 (ζ) Eridani	p	4	35	ſ	0	6	1	eF. ſtellar. about 1′ nf. II. 286.
592	20	Neb. II. 447.	p	0	6	ſ	0	2	1	Two. The p. vF. vS. The next eF. eS. and left doubtful.
593				0	0		0	5		
594	—	26 Ceti	f	18	21	ſ	0	23	1	vF. mE. bM. 3′½ l. 1′½ b.
595	21	29 ——	p	28	42	n	1	17	1	vF. S. ſome ſt. in it.
596	—	44 Hydræ	p	34	21	n	0	50	2	vF. S. lbM. ſf. a trapezium of S. ſt.
597	24	— ——	p	59	13	n	2	44	1	vF. S. R. vglbM.
598	30	59 (c) Leonis	f	2	40	ſ	1	19	1	eF. S. lE. not verified.
	1787									
599	Jan. 11	55 (δ) Gemini	f	68	4	ſ	0	12	1	eF. pL. r.
600	14	30 (η) Leonis	p	11	47	ſ	0	20	1	vF. S. iR.
601	—	— — ——	p	11	4	n	0	3	1	vF. cS. lE. er.
602	—	29 Comæ	p	12	7	n	0	6	1	vF. cL. vgbM. ſ. cBſt.
603	—	— ——	p	6	16	n	0	11	1	vF. E. np ſf. 2′½ l.
604	17	58 Androm.	f	2	45	ſ	0	21	1	vF. ſtellar. confir. 240.
605	Feb. 10	9 (1ſt μ) Canc	p	3	15	n	0	46	1	vF. S. iF.
606	13	10 (2d μ) ——	f	11	31	ſ	1	1	2	vF. S. ſtellar.
607	—	33 (η) ——	p	12	33	n	0	38	1	vF. vS.
608	22	69 (ν) ——	f	2	5	n	0	33	1	eF. S. R. vlbM.
609	—	21 (θ) Crater	f	2	28	n	0	28	1	vF. vS. R. with 240 gbM.
610	—	65 Virginis	p	33	51	ſ	0	12	1	eF. pL. E.
611	—	— ——	p	32	29	n	0	50	1	vF. S. no time to verify.
612	March 11	87 (e) Leonis	f	23	23	ſ	0	57	1	vF. cS. E.
613	—	44 (k) Virgin	p	1	42	n	0	8	1	vF. E. er.
614	—	— — ——	f	0	57	ſ	0	49	1	oF. S. iR.
615	17	38 Leo. min.	p	1	27	ſ	0	27	2	cF. S. er.
616	—	6 Canum	p	34	40	ſ	1	1	2	vF. cL. iF. 4′ dia. 5′ ſ. ſt. 6 m.
617	—	— ——	p	27	3	ſ	1	13	2	eF. pL. iR. 1′ dia. or more.
618	—	12 ——	p	3	8	ſ	1	31	1	eF. vS.
619	—	17 ——	f	11	29	n	0	2	1	vF. S. E. nearly mer.
620	—	— ——	f	26	36	ſ	0	14	1	cF. E. nearly par. r. ¾′ l.
621	—	— ——	f	38	29	ſ	0	46	1	vF. S. iR. conf. 300.
622	—	12 (λ) Coronæ	f	7	17	ſ	0	37	1	vF. S. R. diſcov. in gaging.
623	—	— — ——	f	24	7	ſ	0	19	1	vF. vS. n. 2 ſt. 300 confir.
624	—	— — ——	f	27	24	ſ	0	9	1	vF. S. bM. diſcov. with 300.
625	18	10 (n) Urſæ	p	2	41	ſ	2	27	1	vF. vS. 300.
626	—	— — ——	f	7	14	ſ	0	4	2	vF. S. iF. lbM. r.

III.	1787	Stars.		M.	S.		D.	M.	Ob	Deſcription.
627	March 18	43 Lyncis	p	17	50	ſ	1	2	2	vF. vS. ſtellar. 300.
628	—	— —	p	16	48	ſ	0	9	1	cF. cS.
629 630	—	— —	p	15	24	ſ	0	8	1	Two. Both vF. vS. diſt. 3′. nearly mer. 300.
631	—	34 (μ) Urſæ	f	3	39	ſ	1	55	2	vF. S. R. 300.
632	—	47 —	p	2	8	n	0	32	2	cF. vS. lE. mer. gmbM.
633	—	20 Canum	f	1	58	ſ	0	13	1	vF. S. lbM.
634	—	54 (φ) Bootis	p	1	24	ſ	0	36	1	vF. vS. conf. 300 ſp. 2 vBſt.
635 636	—	— — —	f	6	42	n	0	46	1	Two. The nf. vF. vS. verif. 300. The ſp. diſcov. with 300 eF. S. iF.
637	—	30 (g) Hercul	p	24	18	ſ	1	5	1	vF. eS. 300. ſhewed a vSſt. with nebu.
638	—	— — —	p	3	33	ſ	0	59	1	vF. vS.
639	—	— — —	p	2	53	ſ	1	24	1	eF. eS.
640	—	— — —	f	0	54	ſ	1	5	1	vF. vS.
641	—	— — —	f	1	10	ſ	1	16	1	vF. vS.
642	19	70 Virginis	f	1	37	ſ	0	26	1	vF. S. iF. time l. inaccurate.
643	—	— —	f	3	43	n	0	5	1	vF. S. lE. juſt ff. ſt.
644	—	5 (υ) Bootis	f	25	41	ſ	0	44	1	vF. vS. E. confir. 300.
645	—	30 (ζ) —	p	10	16	n	0	17	1	eF. vS. lbM. betw. 2 vFſt. 300.
646	—	28 (β) Serpen	f	11	15	n	0	24	1	vF. S. lE.
647	20	44 Lyncis	p	33	11	ſ	2	16	1	vF. vS. verif. 300.
648	—	13 Canum	p	36	41	n	0	46	1	vF. E. par. 1′ l.
649	—	— —	f	12	29	n	1	0	1	vF. S. lE.
650	—	— —	f	19	54	n	2	18	1	eF. vS.
651	—	— —	f	27	11	n	1	11	1	vF. S.
652	—	— —	f	29	47	n	0	17	1	eF. vS.
653	—	— —	f	62	59	n	1	34	1	vF. pS. E. mer. 300.
654	April 9	19 —	p	7	36	n	0	51	1	vF. vS. lbM.
655	—	— —	p	*	26	n	2	57	1	vF. pS. lbM. *forgot, but is 5, 6, or 7′.
656	—	20 —	f	15	21	n	1	15	1	vF. vS. lbM.
657 658	—	— —	f	83	33	n	1	57	1	Two. Both vF. vS. E. in differ. directions. 2 or 3′ diſt. par. each ſ. Sſt.
659	—	— —	f	117	16	n	0	18	1	vF. vS. r.
660	—	— —	f	119	18	n	1	16	1	eF. cS.
661	—	— —	f	125	25	n	0	44	1	eF. S.
662	11	29 (γ) Virgin	f	2	8	n	0	54	1	vF. pL.
663	—	— — —	f	3	33	n	0	55	1	vF. S. iF.
664	—	— — —	f	6	6	ſ	0	14	1	vF. S.
665	15	90 (p) —	p	1	48	n	0	23	2	cF. cL. R. vlbM. r. 5′ dia.
666	—	102 (1ſt υ) —	p	19	18	ſ	0	55	1	eF. vS.
667	—	— — —	p	18	53	ſ	0	33	2	eF. vS. verif. 300. 2d obſ. vF. S.
668	—	105 (φ) —	p	3	29	ſ	0	58	1	cF. S. r.
669	May 7	61 —	p	5	42	n	1	30	1	vF.
670	—	— —	p	2	45	n	1	46	1	vF.
671	—	8 Libræ	p	9	38	ſ	1	28	1	cF. S. R. ſp and joining 2 Sſt.

III.	1787	Stars.		M.	S.		D.M.		Ob	Deſcription.
672	May 12	19 (λ) Bootis	p	48	58	n	0	41	3	cF. vS. ſtellar. 300.
673	—	— — —	p	38	30	n	2	21	1	cF. S. R. or lE.
674	—	— — —	p	5	52	n	2	31	1	cF. cS. iR.
675	—	38(2d *b*) —	p	11	18	n	0	35	1	vF. pS. iF. ſp. 2 S. unequal ſt.
676	15	24 (*g*) —	p	16	34	n	0	35	2	cF. cS. lE nearly par.
677	—	— — —	p	2	56	ſ	1	15	1	vF. pS. lE.
678 }	—	*A Bootis 7m	f	0	3	n	0	3	1	{ Two. The p. vF. vS. The f.
679 }				0	48		0	1		eF. eS. * See note.
680	—	42 Herculis	p	13	17	n	1	0	2	vF. S. R. lbM. er. near ſome Sſt.
681	16	*C Canᵐ 6m.	f	0	44	ſ	0	15	1	cF. vS. lE. * See note.
682	—	— — —	f	7	35	ſ	0	7	1	eF. cS. E. ſp. Sſt.
683	—	— — —	f	12	35	ſ	0	25	1	cF. pL. iF.
684	—	— — —	f	13	49	n	0	34	1	vF. vS. R.
685	—	27 (γ) Bootis	p	19	48	n	1	6	2	vF. cS. R. ſbM.
686	—	— — —	f	8	42	n	0	18	1	eF. cS. lbM.
687	—	— — —	f	13	27	n	0	23	1	cF. pS. another ſuſpec. 2′ n. 300.
688	—	16(τ) Coronæ	f	7	34	ſ	0	48	1	vF. cS. iR.
689	—	67 (π) Hercu	p	20	32	ſ	0	11	1	eF. cL. iE. nearly par.
690	19	10 Libræ	p	4	6	ſ	0	43	1	vF. cS. iF. lbM.
691	—	— —	f	6	51	ſ	0	59	1	cF. ſmbM. ſtellar.
692	Aug. 12	33 (ι) Aquarii	p	5	23	n	0	38	1	eF. E. np ſf. 2′ l. 1′ b.
693	Sept. 11	41 —	p	11	30	n	0	36	1	eF. vS. 360 confirmed it.
694	Oct. 11	50 (*f*) Caſſio	f	90	22	n	0	30	1	vF. vS. iR. bM.
695	Nov. 3	10 Camelop	p	155	0	n	0	53	1	eF. pL. iF.
696	5	17 (ξ) Cephei	p	16	35	ſ	0	47	2	vF. S. R. lbM. r. 1′ dia.
	1788									
697	Jan. 14	67 Urſæ	f	11	9	n	0	40	3	vF. E. np ſf. 5′ l. 1′ b.
698	—	27 (γ) Bootis	p	39	53	n	1	29	2	vF. S.
699	—	— — —	p	38	20	n	2	8	2	vF. S. iF.
700	Feb. 3	45 (ω) Urſæ	p	14	53	ſ	1	33	1	cF. L. iE. mb. ſ. M. 4′ l. 2′½ b.
701	—	— — —	p	6	6	ſ	0	1	1	vF. vS. iF.
702	—	13 Canum	p	42	13	ſ	0	55	1	vF. vS.
703	5	71 (ο) Gemin.	p	10	3	ſ	0	46	1	vF. vS. perhaps a patch of ſt.
704	—	60 Urſæ	p	81	11	ſ	0	22	1	eF. vS. perhaps a patch of Sſt.
705	—	— —	p	39	57	ſ	0	43	1	vF.
706	—	— —	p	23	49	n	0	38	2	vF. vS. lE. ſ. cBſt.
707	—	63 (χ) —	f	11	2	n	0	34	2	vF. vS. another ſuſp. ſf. eF. eS.
708	6	59 —	f	30	14	ſ	0	31	1	vF. vS. in a line with 2 ſt. nf ſp.
709	March 9	21 Lyncis	f	34	50	n	1	41	1	vF. R. vgbM. 2′½ dia.
710	—	9 (ι) Urſæ	p	45	51	n	0	49	1	vF. iF. 2′½ l. 1′¾ b.
711	—	— — —	p	41	10	n	1	49	1	eF. E. ſp nf. 3′½ l. 2′½ b.
712	—	— — —	p	4	49	n	1	6	1	eF. cS. r. p. ſome Fſt.
713	—	— — —	f	25	7	n	1	15	1	cF. cS. lE.
714	—	— — —	f	24	58	n	1	11	1	cF. cS. lE.
715	—	63 (χ) —	f	3	26	n	0	39	1	eF. pS.

III.	1787	Stars.		M.	S.		D.	M.	Ob	Deſcription.
716	March 9	63 (χ) Urſæ	f	5	2	n	2	26	1	vF. vS.
717	—	3 Canum	p	14	1	n	0	37	1	cF. mE. nearly mer. 5′ l.
718	—	— —	p	4	6	ſ	0	51	1	vF. vS.
719 720	—	— —	p	2	47	ſ	1	31	1	Two. Both vF. vS. diſt. 1′ in the ſame meridian.
721	—	— —	f	32	1	ſ	1	21	1	vF. S.
722	11	49 (g) Virgin	p	18	9	ſ	0	21	1	eF. S.
723	April 1	Neb. II. 728.	p	0	25	ſ	0	2	1	eF. vS.
724	8	61 Virginis	f	1	43	ſ	2	23	1	cF. vS. iF.
725	10	60 Urſæ	f	39	40	ſ	1	14	2	eF. cL. iR. lbM. 3′ dia.
726	—	— —	f	42	45	ſ	0	34	2	vF. pS. R.
727	12	35 (σ) Hercul	f	16	11	n	0	14	1	cF. S. E. par.
728	13	42 —	f	20	46	n	0	54	1	vF. cS. iR.
729	27	19 (λ) Bootis	p	113	28	ſ	0	3	1	vF. S.
730	28	27 (β) Hercul	f	4	6	n	0	2	1	eF. vS. E.
731	29	27 (γ) Bootis	p	15	47	n	1	16	1	vF. vS.
732	—	— — —	p	15	33	n	1	22	1	vF. vS. lE.
733	—	— — —	p	9	25	n	2	4	1	vF. vS.
734	—	— — —	p	8	52	n	2	8	1	cF. pS.
735	—	22 (τ) Hercul	f	30	17	ſ	1	2	1	eF. pS. with 300 iF.
736	30	21 (1ſt.) Libr	f	7	7	n	1	59	1	vF. pL. E. mer. lbM. 300.
737	May 1	23 (θ) Bootis	f	49	59	ſ	1	46	1	vF. vS. ſtellar.
738	25	12 (ι) Draco	f	17	8	n	0	44	1	vF. vS.
739	June 2	14 (η) —	p	32	30	n	0	57	1	vF. R. vgbM. er. 3′ dia.
740	3	15 (A) —	p	10	14	ſ	3	21	1	cF. pL. iE.
741	6	31 (1ſt.) —	p	5	13	ſ	0	5	1	eF. ſtellar. with 300 lE. par.
742	July 8	*B Draco 7m.	f	4	25	ſ	0	27	1	vF. ſtellar. verif. 300. * See note.
743	30	19 Aquilæ	f	9	24	n	0	26	1	cF. iR. r. 3 or 4′ dia.
744	Aug. 2	51 —	p	8	8	n	0	29	1	vF. pL. R. vgmbM.
745	Nov. 1	27 (δ) Cephei	f	26	10	ſ	1	24	1	vF. pL. iF. er.
746	—	36 Camelop.	f	64	5	ſ	0	38	1	vF. S. R. lbM.
747	Dec. 3	*22 Cam Hev	p	37	1	ſ	0	8	1	cF. pL. iF. mbM. er. ſome ſt. viſible. * See note.

Fourth claſs. Planetary nebulæ.

Stars with burs, with milky chevelure, with ſhort rays, remarkable ſhapes, &c.

IV.	1785	Stars.		M.	S.		D.	M.	Ob	Deſcription.
30	May 1	14 Canum	p	6	48		0	55	2	Two ſt. diſt. 3′ connected with a vF. narrow nebuloſity.
31	Oct. 3	50 Aquarii	f	7	55		0	37	1	F. S. ſtellar. with pL. chev.
32	5	62 (b) Eridani	f	0	35	n	0	21	2	vB. vS. mbM. like a ſt. affected with irregular burs.
33	—	49 (d) Orion	p	2	33	n	0	28	4	A ſt. with m. chev. or vBN. with m. nebuloſity.

IV.	1785	Stars.		M.	S.		D.M.		Ob	Deſcription.
34	Dec. 28	40 (2d φ) Orio	f	5	41	ſ	0	12	2	cB. S. nearly R. like a ſt. with L. dia. with 240 like an ill defined planetary neb.
35	31	9 Hydræ	p	8	19	ſ	0	14	1	A S ſt. with a bruſh ſp. FS. it reſembles fig. 7. Phil. Tranſ. Vol. LXXIV. Tab 17.
	1786									
36	Jan. 1	60 Orionis	p	11	38	ſ	0	20	3	A ſt. affected with vF. extenſive m. chev. The ſt. not quite central.
37	Feb. 15	28 (ω) Draco	f	20	33	ſ	2	12	1	A planetary neb. vB. has a diſk of about 35″ dia. but very ill defined edge. With long attention a vB. well defined R. center becomes viſible.
38	24	55 Orionis	f	18	3	n	1	17	2	A cſt. affected with vF. m. chev.
39	March 19	2 Navis	p	3	32	ſ	0	5	1	pB. R. r. within the 46th of the *Connoiſſ. des Temps* almoſt of an equal light throughout 2′ dia. no connection with the cluſter, which is free from nebuloſity.
40	27	68 (ι) Virgin	p	30	45	ſ	0	18	1	A pBſt. with a ſeeming bruſh to it np. may be a vS neb. cloſe to it.
41	May 26	14 Sagittarii	p	11	58	ſ	1	15	1	A double ſt. with extenſive nebuloſity of different intenſity. About the double ſt. is a black opening reſembling the neb. in Orion in miniature.
42	Sept. 30	51 Ceti	f	7	26	n	0	27	1	A ſt. about 8 or 9 m. with vF. bran. mer. each branch 1′ l.
43	Oct. 17	26 (β) Perſei	p	2	48	n	1	54	2	A pBſt. with 2 F. branches.
44	Nov. 28	5 Monocero	p	7	16	ſ	0	2	ſ	A ſt. involved in m. chev.
	1787									
45	Jan. 17	55 (δ) Gemin	f	9	6	ſ	1	1	2	A ſt. 9 m. with a pB. m. nebuloſity. equally diſperſed all around. A very remarkable phænomenon.
46	Feb. 22	99 (ι) Virgin	p	4	38	n	0	57	1	pB. almoſt cB. vS. ſtellar. like a ſtar with burs.
47	March 11	44 (ι) ——	f	1	48	ſ	0	46	1	pB. ſtellar. reſembles a ſt. with a bur all around.
48	18	19 Leo. min	f	6	32	ſ	0	17	1	A vFſt affected with vF. nebuloſity. E. ſp nf. 1′ l. 3co.
49	April 15	102 (1ſt υ) Vir	p	6	9	ſ	0	52	2	pB. ſtellar. like a ſt. with a S. bur all around.
50	May 12	77 (κ) Hercul	p	40	13	ſ	0	28	1	vB. R. 4′ dia. almoſt equally B. with a F. r. margin.

IV.	1787	Stars.		M.	S.		D.M.		Ob	Deſcription.
51	Aug. 8	61 (*g*) Sagitt	p	13	56	n	1	23	2	A cB. S. beautiful planetary nebula; but c. hazy on the edges, of a uniform light; 10 or 15″ dia. perfectly R. I ſhewed it to M. De la Lande.
52	Nov. 3	4 (*d*) Caſſio	p	4	0	ſ	1	6	2	A ſt. 9 m. with vF. nebuloſity of S. extent about it.
53	—	10 Camelop	p	55	42	n	0	11	2	A pB. planetary nebula. near 1′ dia. R. of uniform light and pretty well defined. 2 obſ. with 360 magnified in proportion; but ſtill pretty abruptly defined, and a little elliptical.
	1788									
54	Jan. 14	67 Urſæ	f	7	32	ſ	0	30	1	cB. S. N. with F. chev.
55	Feb. 6	34 Lyncis	p	28	4	n	0	2	2	pB. R. almoſt of an even light throughout, approaching to planetary, but ill defined and a little fainter on the edges ¾ or 1′ dia. p. 1′ pc ſt.
56	—	59 Urſæ	f	25	11	n	0	56	1	cB. iR. cBNM. with extenſive chev. 5′ dia.
57	June 11	35 (σ) Hercul	f	34	27	ſ	0	18	2	A vS. F. ſt. involved in eF. nebuloſity.
58	Nov. 25	24 Cephei	f	116	28	n	0	2	1	A ſt. 9 m. ſurrounded with vF. m. nebuloſity. The ſt. is either double, or not R. Leſs than 1′ dia.

Fifth claſs. Very large nebulæ.

V.	1785	Stars.		M.	S.		D.M.		Ob	Deſcription.
25	Nov. 27	18 Ceti	f	1	30	n	1	2	1	Four or five pL. ſt. forming a trapezium of about 5′ dia. The incloſed ſpace is filled up with faintly terminated m. nebuloſity. The ſt. ſeem to have no connexion with the nebuloſity.
26	Dec. 7	18 Leo. min.	p	8	7		1	1	2	cB. mE. par. 8′ l. 3′ b.
27	26	15 Monocero	p	0	12	ſ	0	6	2	Some pBſt. 7 or 8′ ſp. 15th Monoc. are involved in eF. m. nebuloſity which loſes itſelf imperceptibly.
	1786									
28	Jan. 1	48 (σ) Orion	f	2	46	n	0	44	2	Remarkable m. nebuloſity, divided in 3 or 4 large patches, including a dark ſpace; cannot

V.	1786	Stars.		M.	S.		D.M.		Ob	Deſcription.
										take up leſs than ½ degree, but I ſuppoſe it to be much more extenſive.
29	2	61 Urſæ	f	45	38	f	0	40	1	eF. vL. vlbM. r. 10′ l. 8 or 9′ b.
30	18	42 45 } c Orioni	p	0	0	n	0	0	2	The 1ſt and 2d c Orionis, and the ſtars about them, are involved in eF. unequally B. m. nebuloſity.
31	31	44 (ι) ——	p	0	0	n	0	0	2	ι Orionis with its neighbouring ſt. are involved in eF. m. nebuloſity to a great extent.
32	Feb. 1	28 (η) ——	p	17	26	f	1	4	2	cB. vL. m. diffuſed and vaniſhing. near and ſf. Bſt.
33	—	——	f	1	26	f	0	7	1	Diffuſed eF. m. nebuloſity. The means of verifying this phænomenon are difficult.
34	—	46(ε) Orionis	p	0	0	n	0	0	1	I am pretty certain ε Orionis is involved in unequally diffuſed m. nebuloſity.
35	—	36 (υ) —— 56 ——	f p	3 2	39 16	f n	0 0	40 28	4	Diffuſed m. nebuloſity, extending over no leſs than 10 degrees of PD. and many degrees of RA. It is of very different brightneſs, and in general extremely F. and difficult to be perceived. Moſt probably the nebuloſities of the 28th, 30, 31, 33, 34, and 38th of this claſs are connected together, and form an immenſe ſtratum of far diſtant ſtars, to which muſt alſo belong the nebula in Orion.
36	Oct. 17	35 (ν) Andro	p	9	8	f	0	20	2	vF. vL. E. nearly mer. or a little from np ſf. about 20′ l.
37	24	57 Cygni	f	5	1	f	1	1	1	vL. diffuſed nebuloſity. bM. 7 or 8′ l. 6′ b. and loſing itſelf vg. and imperceptibly.
38	Dec. 20	19 (β) Orion	f	11 11	9 35	n f	1 0	19 52	1	Strongly ſuſpected nebuloſity of v. great extent. Not leſs than 2°11′ of PD. and 26″ of RA. in time.
39	21	11 (β) Crater	p	8	15	f	0	17	2	vF. mE. nearly par. or about 10° ſp nf. vgbM. 8′ l. 3′ b.

V.	1786	Stars.		M.	S.		D.M		Ob	Deſcription.
40	Dec. 21	11 (β) Crater	p	7	49	f	0	26	2	vF. mE. 15° ſp nf. vlbM. about 7′l. 4′b.
41	1787 March 17	6 Canum	p	8	27	f	1	12	1	vB. E. 60° ſp nf. 20′l. 2′b.
42	20	13 ——	p	18	39	n	1	48	1	vB. mE. ſp nf. but nearly par. mbM. 16′l.
43	1788 March 9	3 ——	p	0	38	f	1	41	3	v brilliant. BN. with Fm. bran. np ſf. 15′l. and to the ſf. running into vF. nebuloſity extending a great way. the N. is not R.
44	Nov. 1	36 Camelop	f	84	33	n	0	23	2	cB. R. vgbM. BN. 6 or 7′ dia. with a F. branch extending a great way to the np. ſide; not leſs than ½ degree. and to the n. or nf. the nebuloſity diffuſed over a ſpace not leſs than a whole degree.

Sixth claſs. Very compreſſed and rich cluſters of ſtars.

Additional abbreviations.	Cl. Cluſter.	com. compreſſed.
	ſc. ſcattered.	co. coarſely.

VI	1785	Stars.		M.	S.		D.M.		Ob	Deſcription.
20	Oct. 27	18(ι)Piſ.Auſt	f	133	24	n	0	23	2	cB. iR. 8 or 9′ dia. a great many of the ſt. viſible, ſo that there can remain no doubt but that it is a Cl. of vS. ſtars.
21	Dec. 7	25 Gemino	f	2	15	f	2	15	1	A v. rich and v. com. Cl. ſt. of about 5′ dia. ſome of the largeſt ſt. are in a row.
22	1786 Feb. 1	31 Monocero	p	30	4	n	2	20	4	A beautiful Cl. of much com. ſt. conſid. rich. 10 or 12′ dia. C. H. diſcovered it in 1783.
23	June 27	46 (υ) Sagitt	p	49	15	f	0	42	1	A beautiful Cl. of vS. ſt. of various ſizes. 15′ dia. very rich.
24	Oct. 17	58 (ν) Cygni	f	15	56	n	1	18	2	A v. com. and v. rich Cl. of cSſt. about 6′ l. 4′ b. nearly par.
25	Dec. 11	27 (κ) Perſei	f	5	55	n	2	25	2	A beautiful com. and rich Cl. of S. and L. ſt. 7 or 8′ dia. the L. ſt. arranged in lines like interwoven letters.

VI.	1786	Stars.		M.	S.		D.	M.	Ob	Deſcription.
26	Dec. 11	53 (*d*) Perſei	f	13	34	f	1	13	1	A vF. and v. com. Cl. of eS. ſt. near 4′ dia.
27	27	22 Monocero	p	20	9	n	0	51	1	A v. beautiful Cl. of much com. S. and L. ſt. above 20′ dia.
28	1787 Jan. 11	75 (*l*) Orionis	f	21	25	n	1	2	1	A Cl. of e. com. and eS. ſt. c. rich iF. the f. and moſt com. part R.
29	Oct. 14	3 Lacertæ	p	7	52	n	2	7	1	A com. Cl. of eS. ſt.
30	18	7 (ϱ) Caſſiop	f	3	10	f	0	46	3	A beautiful Cl. of v. com. Sſt. v. rich. C. H. diſcovered it 1783.
31	Nov. 3	37 (δ) ——	f	19	48	n	1	2	1	A beautiful Cl. of pL. ſt. near 15′ dia. conſ. rich.
32	1788 Sept. 21	80 (1ſt π) Cyg	p	11	26	n	0	28	1	A beautiful Cl. of p. com. ſt. 8 or 9′ dia. nearly R. c. rich.
33	Nov. 1	7 (χ) Perſei	f	1	7	f	0	22	1	A v. beautiful and brilliant Cl. of L. ſt. v. rich. the M. contains a vacancy.
34		—— ——	f	4	0	f	0	23	1	A v. beautiful, brilliant Cl. of L. ſt. iR. v. rich. near ½ degree in dia.
35	26	15 (κ) Caſſiop	p	1	22	f	1	26	1	A S. Cl. of vF. and e. com. ſt. about 1′ dia. The next ſtep to an er. neb.

Seventh claſs. Pretty much compreſſed cluſters of large or ſmall ſtars.

VII.	1785	Stars.		M.	S.		D.	M.	Ob	Deſcription.
18	July 17	12 Vulpeculæ	p	7	56	n	0	44	1	An E. Cl. of i. ſc. ſt. of various ſizes. c. rich.
19	30	21 Aquilæ	p	5	49	n	1	55	1	A p. com. Cl. of p. ſc. ſt. of var. ſizes, magnitudes, and colours. iF. and unequally com. 12 or 15′ dia.
20	Nov. 1	7 Monocero	f	1	3	n	0	35	3	A beautiful Cl. of p. com. and equally ſc. ſt. 10 or 12′ dia.
21	Dec. 26	109 (*n*) Tauri	p	14	59	n	1	37	1	A Cl. of p. com. ſt. with many eS. ſt. mixed with them.
22	28	13 Monocero	f	2	48	n	0	21	1	A S. Cl. of p. com. vS. ſt.
23	30	31 (η) Canis	f	32	6	f	0	39	1	A com. Cl. of pL. ſt. c. rich.
24	1786 Jan. 1	60 Orionis	p	5	9	f	0	9	2	A Cl. of p. com. pS. ſc. ſt. with many eS. ſuſpec. betw. them 7 or 8′ dia.
25	27	8 Monocero	p	11	46	n	0	49	1	A Cl. of p. com. ſt. of ſeveral ſizes 4 or 5′ dia. with extenſively ſtraggling ones.

VII.	1786	Stars.		M.	S.		D.	M.	Ob.	Deſcription.
26	Jan. 30	6 Monocero	f	8	59	n	1	7	1	A Cl. of eS. and pm. com. ſt. with a few L. but not rich. in the ſhape of a hook.
27	Feb. 24	11 ——	f	42	13	f	1	21	2	An i. Cl. of eS. ſt. c. com. 9 or 10′l. 4 or 5′ b. with an extending bran. towards ſp. C. H. diſcov. 1783.
28	Mar. 19	2 Navis	p	8	43	n	0	47	1	A Cl. of pS. ſt. p. rich. 15′ dia.
29	April 30	5 (ρ) Scorpii	p	7	14	n	0	38	1	A Cl. of vS. ſt. p. rich 6′ l. 4′ b. in the form of a parallelogram.
30	May 26	14 Sagittarii	p	1	35	n	0	9	1	A Cl. of pS. ſc. ſt. above 15′ dia.
31	——	——	f	1	29	f	0	25	1	A Cl. of vS. and p. com. ſt. c. rich. 2 or 3′ dia.
32	Sept. 21	58 Androm	p	10	49	f	0	8	4	A vL. co. ſc. Cl. of vL. ſt. iR v. rich. takes up ½ degree like a nebulous ſt. to the naked eye.
33	Oct. 18	11 (μ) Aurig	f	6	32	n	0	54	1	A Cl. of p. com. pS. Sſt. c. rich. contains 1 L. the reſt are all of a ſize.
34	Dec. 11	13 (α) ——	f	9	7	n	0	32	1	A Cl. of vF. and vSſt. p. com. but not rich. iF. 3′ dia.
35	24	70 (ξ) Orionis	f	15	53	f	1	29	1	A Cl. of S. pm. com. ſt. with ſuſpected m. nebuloſity.
36	26	18 Monocero	p	3	48	n	1	0	1	A Cl. of v. ſc. ſt. c. rich. and of great extent.
37	27	77 Orionis	f	12	24	n	0	55	1	A Cl. of v. com. eSſt. c. rich. 3 or 4′ dia. moſt com. M.
38	——	22 Monocero	p	7	39	n	1	31	2	A beautiful Cl. of vSſt. of ſeveral ſizes. c. com. and rich M. 10 or 12′ dia.
39	1787 Jan. 17	21 (σ) Aurigæ	f	3	25	f	2	6	1	A p. com. Cl. of Sſt. 4′ dia.
40	Oct. 14	3 Lacertæ	p	38	31	n	1	35	1	A Cl. of Sſt. of ſeveral ſizes. 3 or 4′ dia. p. rich. like a forming one.
41	——	——	f	5	8	n	0	2	2	A S. Cl. of ſt. p. com. e. rich in vS. ſt. The com. part 4 or 5′ dia.
42	18	24 (η) Caſſio	f	29	41	n	0	26	2	A brilliant Cl. of L. and vS. ſt. c. rich.
43	Nov. 3	1 (ι) ——	p	11	41	n	1	25	1	A S. Cl. of vSſt. c. com. and p. rich.
44	——	—— ——	f	4	34	n	1	8	2	A Cl. of p com. pLſt. c. rich. The ſt. arranged chiefly in lines from ſp. nf.
45	——	37 (δ) ——	p	9	29	f	1	28	2	A S. p. com. Cl. of ſt. not rich. iF. like a forming one.
46	——	—— ——	f	17	23	n	1	44	2	A S. Cl. of pL. ſt. c. rich.
47	——	10 Camelop	p	55	40	n	1	37	2	A Cl. of ſt. p. rich and c. com. lE. 3 or 4′ dia. iF.

VII.	1787	Stars.		M.	S.		D.	M.	Ob	Deſcription.
48	Nov. 9	32 Caſſiop	f	17	1	ſ	1	40	1	A com. Cl. of ſome pL. and many vS. ſt. iR. 6 or 7′ dia.
49	—	45 (ι) —	p	11	8	n	0	20	1	A Cl. of ſome cL. ſt. and many eS. ſo as hardly to be ſeen. The Lſt. arranged in circular order 3 or 4′ dia.
	1788									
50	Sept. 27	81 (2d π) Cyg	p	22	13	ſ	1	14	1	A few Sſt. with ſuſpected nebuloſity, with 300 many vS. ſt. intermixed with the former, ſo as to make a Cl.
51	Oct. 19	71 (g) —	p	5	49	ſ	0	9	1	A p. com. Cl. of pS. ſt. c. rich iR. 5 or 6′ dia.
52	—	— — —	p	0	42	n	0	34	1	An extenſive Cl. of Lſt. c. rich above 20′ dia.
53	—	73 (ρ) —	f	30	41	n	0	48	2	A L. Cl. of p. com. cLſt. above 15′ dia. c. rich.
54	Nov. 1	36 Camelop	f	29	1	n	0	16	1	A vF. patch. or S. Cl. of eSſt.
55	23	32 (ι) Cephei	f	57	34	n	1	47	3	A Cl. of cS. ſt. iF. p. rich and com. contains a vacancy M.

Eighth claſs. Coarſely ſcattered cluſters of ſtars.

VIII.	1785	Stars.		M.	S.		D.	M.	Ob	Deſcription.
41	Dec. 7	98 (k) Tauri	f	12	11	ſ	0	54	1	A co. Cl. of ſt. or projecting point of the m. way.
42	—	125 —	p	1	22	ſ	0	4	2	A Cl. of co. ſc. ſt. above 15′ dia. The ſt. nearly of a ſize and equally ſc.
43	26	109 (n) —	p	15	30	n	1	29	1	A Cl. of v. co. ſc. Lſt. join. to VII. 21.
44	28	5 (η) Can. min	f	0	38	ſ	1	54	1	A Cl. of v. co. ſc. Lſt. form a croſs, not rich.
45	31	6 Navis	p	32	48	ſ	0	1	1	A co. ſc. Cl. of ſt. not rich.
46	—	— —	p	10	18	n	0	49	1	A vL. but co. ſc. Cl. of ſt.
47	—	— —	p	10	27	n	0	39	1	A Cl. of ſc. ſt. or the m. way crouded with ſt. of equal ſize and colour.
	1786									
48	Jan. 1	78 Orionis	f	10	59	ſ	1	9	1	A Cl. of v. ſc. ſt. of various ſizes. above ½ degree of extent.
49	3	*B Gemi. 6m	p	33	23	n	0	35	1	A Cl. of co. ſc. Lſt. not rich. *See note
50	27	8 Monocero	f	10	58	n	0	49	2	A Cl. of ſt. arranged in a broad row. 25′ l. 6 or 8′ b. not v. com. but p. rich.
51	Feb. 23	11 —	f	25	25	ſ	0	1	1	A Cl. of v. ſc. ſt.
52	Mar. 19	2 Navis	p	12	16	n	1	32	1	A Cl. of vL. co. ſc. ſt. not rich.
53	June 27	46 (υ) Sagittæ	p	82	10	ſ	1	4	1	A Cl. of ſc. Sſt. 8′ dia. not v. rich.

VIII.	1786	Stars.		M.	S.		D.	M.	Ob	Deſcription.
54	June 27	46 (υ) Sagittæ	p	71	19	ſ	0	25	1	A co. ſc. Cl. of cLſt. The place is that of a S. triangle.
55	—	— — —	p	64	17	ſ	0	23	1	A co. ſc. Cl. of Lſt.
56	Oct. 17	37 (γ) Cygni	f	0	53	n	0	32	1	A S. Cl. of co. ſc. ſt. of various ſizes. E. like a forming one.
57	—	58 (ν) ——	f	8	47	n	0	20	1	A Cl. of co. ſc. pS. ſt. of ſeveral ſizes. not rich.
58	24	57 ——	f	3	19	n	0	16	2	A Cl. of pL. ſc. ſt. not v. rich.
59	—	59 Perſei	f	7	59	n	0	21	1	A Cl. of co. ſc. pL. ſt. not v. rich.
60	Nov. 26	19 Monocero	p	5	3	ſ	0	23	1	A Cl of pL. ſc. ſt. not v. rich. may be a projecting point of the m. way.
61	1787 Jan. 17	21 (σ) Aurigæ	p	16	38	ſ	0	30	1	A Cl. of co. ſc. Lſt. iF. not rich. like a forming one.
62	Sept. 19	35 (γ) Cephei	p	4	43	ſ	4	50	2	A Cl. of co. ſc. Lſt. not rich. but the ſt. are brilliant. one 7 m.
63	Oct. 16	21 (ζ) ——	f	1	21	ſ	0	56	1	A S. Cl. of pL. ſt.
64	Nov. 3	27 (γ) Caſſiop	f	11	12	n	0	53	2	A forming cluſter of p. com. ſt. C. H. diſc. 1783.
65	—	37 (δ) ——	f	17	56	n	0	29	2	A S. Cl. of Sſt. not v. rich. C.H. 1783.
66	—	45 (ι) ——	f	47	9	ſ	1	58	2	A Cl. of co. ſc. cLſt. 8 or 10′ dia. one 7 m. near M.
67	9	17 (ξ) Cephei	p	10	0	ſ	2	0	1	A Cl. of co. ſc. L. and S. ſt. 7′ dia. like a forming one.
68	12	41 Aurigæ	p	8	57	n	1	9	1	A S. Cl. of ſc. ſt. not rich one. 7 m. towards the n. but this does not ſeem connected with the Cl.
69	Dec. 3	18 Androm	p	8	59	ſ	1	20	1	A Cl. of co. ſc. pL. ſt. one 8 m. in the ſf. part.
70	1788 Feb. 3	41 (υ) Perſei	f	46	17	n	1	28	1	A Cl. of co. ſc. Lſt. p. rich above 20′ dia.
71	March 4	58 Aurigæ	p	1	22	ſ	0	44	1	A Cl. of co. ſc. pL. ſt. p. rich the place is that of a double ſt. of the 3d claſs.
72	July 30	62 Serpentis	p	27	26	n	0	6	3	A Cl. of co. ſc. Lſt. C. H. 1783.
73	—	59 (ξ) Aquilæ	p	4	2	ſ	0	34	1	A Cl. of co. ſc. ſt. with one pBſt. M.
74	Sept. 21	80 (1ſt π) Cyg	p	34	12	ſ	0	12	1	A Cl. of co. ſc. Lſt. not rich 6′ dia.
75	26	3 Lacertæ	p	7	29	ſ	2	21	2	A Cl. of co. ſc. Lſt. lE. ſp nf. 16′l.
76	27	59 (1ſt ƒ) Cyg	p	4	1	ſ	0	7	1	A ſt. 6 m. ſurrounded by many cſt. forming a brilliant ſc. Cl. the Lſt. not M. but f.
77	Nov. 1	27 (δ) Cephei	f	17	23	ſ	0	22	2	A Cl. of co. ſc. ſt. 8′ dia. C.H. 1787.
78	26	15 (κ) Caſſio	f	10	56	ſ	1	8	2	A Cl. of v. co. ſc. Lſt: take up 15 or 20′. C. H. diſc. 1784.

Notes to ſome nebulæ and cluſters of ſtars.

I. 138. The number refers to De la Çaille's ſouthern catalogue in the Cœlum Auſtrale Stelliferum.

I. 190. A ſtar of the ſixth magnitude, not contained in any catalogue. I have called it C Canum Venaticorum. It follows Fl. 17. Can. Ven. 37′ 34″ in time, and is 0° 2′ more ſouth than that ſtar.

II. 566. See the note to I. 138.

638. See the note to I. 138.

697. See the note to I. 190.

703. A ſtar of the 7th magnitude, not contained in any catalogue. I called it A Ceti. Not having ſettled its place, I can only give it in a coarſe way. RA. about 0 h. 31′ 37″, PD. about 94° 22′.

III. 678. A ſtar of the 7th magnitude, not containen in any catalogue. I have called it A Bootis. It follows Fl. 39 Bootis 6′ 56″ in time, and is 0° 55′ more north.

681. See the note to I. 190.

742. A ſtar of the 7th magnitude, not contained in any catalogue. I have called it B Draconis. Its place very coarſely is RA. 18 h. 47′. PD. 41°¾.

747. See Mr. Wollaston's general catalogue. Zone 20°.

VIII. 49. A ſtar of the 6th magnitude, not contained in any catalogue. I have called it B Geminorum. Not having ſettled its place, I can only give it in a coarſe way, RA. about 6 h. 52′ 4″. PD. about 55° 17′.

P. S. The planet Saturn has a *ſixth ſatellite* revolving round it in about 32 hours, 48 minutes. Its orbit lies exactly in the plane of the Ring, and within that of the firſt ſatellite. An account of its diſcovery with the forty-feet reflector, and a more accurate determination of its revolution and diſtance from the planet will be preſented to the Royal Society at their next Meetings.

WILLIAM HERSCHEL.

XXI. *An Attempt to explain a Difficulty in the Theory of Viſion, depending on the different Refrangibility of Light. By the Rev.* Nevil Maſkelyne, *D. D. F. R. S. and Aſtronomer Royal.*

Read June 18, 1789.

THE ideas of ſight are ſo ſtriking and beautiful, that we are apt to conſider them as perfectly diſtinct. The celebrated EULER, taking this for granted, has ſuppoſed, in the Memoirs of the Royal Academy of Sciences at Berlin for 1747, that the ſeveral humors of the human eye were contrived in ſuch a manner as to prevent the latitude of focus ariſing from the different refrangibility of light, and conſiders this as a new reaſon for admiring the ſtructure of the eye; for that a ſingle tranſparent medium, of a proper figure, would have been ſufficient to repreſent images of outward objects in an imperfect manner; but, to make the organ of ſight abſolutely complete, it was neceſſary it ſhould be compoſed of ſeveral tranſparent mediums, properly figured, and fitted together agreeable to the rules of the ſublimeſt geometry, in order to obviate the effect of the different refrangibility of light in diſturbing the diſtinctneſs of the image; and hence he concludes, that it is poſſible to diſpoſe four refracting ſurfaces in ſuch a manner as to bring all ſorts of rays to one focus, at whatever diſtance the object be placed. He then aſſumes a certain hypotheſis of refraction of the differently refrangible rays, and builds thereon an ingenious theory of an achromatic object-glaſs, compoſed of two meniſcus glaſſes with water between them, with the help of an analytical calculation, ſimple and elegant, as his uſually are.

He has not, however, demonſtrated the neceſſary exiſtence of his hypotheſis, his arguments for which are more metaphyſical than geometrical; and, as it was founded on no experiment, ſo thoſe made ſince have ſhewn its fallacy, and that it does not obtain in nature. Moreover, which is rather extraordinary, it does not account, according to his own ideas, for the very phænomenon which firſt ſuggeſted it to him, namely, the great diſtinctneſs of the human viſion, as was obſerved to me, many years ago, by the late Mr. JOHN DOLLOND, F. R. S. to whom we are ſo much obliged for the invention of the achromatic teleſcope; for the refractions at the ſeveral humors of the eye being all made one way, the colours produced by the firſt refraction will be increaſed at the two ſubſequent ones inſtead of being corrected, whether we make uſe of NEWTON's or EULER's law of refraction of the differently refrangible rays.

Thus EULER produced an hypothetical principle, neither fit for rendering a teleſcope achromatic, nor to account for the diſtinctneſs of the human viſion; and the difficulty of reconciling that diſtinctneſs with the principle of the different refrangibility of light diſcovered by Sir ISAAC NEWTON remains in its full force.

In order to go to the bottom of this difficulty, as the beſt probable means of obviating it, I have calculated the refractions of the mean, moſt, and leaſt refrangible rays at the ſeveral humors of the eye, and thence inferred the diffuſion of the rays, proceeding from a point in an object, at their falling upon the retina, and the external angle which ſuch coloured image of a point upon the retina correſponds to.

I took the dimenſions of the eye from M. Petit, as related by Dr. Jurin; and, the ſpecific gravities of the aqueous and vitreous humors having been found to be nearly the ſame with that of water, and the refraction of the vitreous humor of an ox's eye having been found by Mr. Hawksbee to be the ſame as that of water, and the ratio of refraction out of air into the cryſtalline humor of an ox's eye having been found by the ſame accurate experimenter to be as 1 to ,68327, I took the refraction of the mean refrangible rays out of air into the aqueous or vitreous humor, the ſame as into water, as 1 to ,74853, or 1,33595 to 1; and out of air into the cryſtalline humor as 1 to ,68327, or 1,46355 to 1. Hence I find, according to Sir Isaac Newton's two theorems, related at Part II. of Book I. of Optics, p. 113. that the ratio of refraction of the moſt, mean, and leaſt refrangible rays at the cornea ſhould be as 1 to ,74512, ,74853 and ,75197; at the fore-ſurface of the cryſtalline as 1 to ,91173, ,91282, and ,91392; and at the hinder ſurface of the cryſtalline as 1 to 1,09681, 1,09550, and 1,09420.

Now, taking with Dr. Jurin 15 inches for the diſtance at which the generality of eyes in their mean ſtate ſee with moſt diſtinctneſs, I find the rays from a point of an object ſo ſituate will be collected into three ſeveral foci, *viz.* the moſt, mean, and leaſt refrangible rays at the reſpective diſtances behind the cryſtalline ,5930, ,6034, and ,6141 of an inch, the focus of the moſt refrangible rays being ,0211 inch ſhort of the focus of the leaſt refrangible ones.

Moreover, aſſuming the diameter of the pencil of rays at the cornea, proceeding from the object at 15 inches diſtance, to be $\frac{1}{5}$th of an inch in a ſtrong light, which is a large allowance for it, the ſemi-angle of the pencil of mean refrangible rays at their concourſe upon the retina will be 7° 12′, whoſe tangent to the radius unity, or ,1264 multiplied into ,0211 inch, the interval of the foci of the extreme refrangible rays, gives ,002667 inch for the diffuſion of the different coloured rays, or the diameter of the indiſtinct circle upon the retina. Now, I find, that the diameter of the image of an object upon the retina is to the object as ,6055 inch to the diſtance of the object from the center of curvature of the cornea; or the ſize of the image is the ſame as would be formed by a very thin convex lens, whoſe focal diſtance is ,6055 inch, and conſequently a line in an object which ſubtends an angle of 1′ at the center of the cornea will be repreſented on the retina by a line of $\frac{1}{5678}$th inch. Hence the diameter of the indiſtinct circle on the retina before found, ,002667 will anſwer to an external angle of ,002667 × 5678′ = 15′ 8″, or every point in an object ſhould appear to ſubtend an angle of about 15′, on account of the different refrangibility of the rays of light.

I ſhall now endeavour to ſhew that this angle of ocular aberration is compatible with the diſtinctneſs of our viſion. This aberration is of the ſame kind as that which we experience in the common refracting teleſcope. Now, by computation from the tabular apertures and magnifying powers of ſuch teleſcopes, it is certain that they admit of an angular indiſtinctneſs at the eye of no leſs than 57′; therefore the ocular aberration is near four times leſs than in a common refracting teleſcope, and conſequently the real indiſtinctneſs, being as the ſquare of the angular aberration, will be 14 or 15 times leſs in the eye than in a common refracting teleſcope, which may be eaſily allowed to be imperceptible.

Moreover, Sir Isaac Newton has obſerved, with reſpect to the like difficulty of accounting for the diſtinctneſs with which refracting teleſcopes repreſent objects, that the erring rays are not ſcattered uniformly over the circle of diſſipation in the focus of the object-glaſs, but collected infinitely more denſely in the center than in any other part of the circle, and in the way from the center to the circumference grow continually rarer and rarer, ſo as at the circumference to become infinitely rare; and by reaſon of their rarity are not ſtrong enough to be viſible, unleſs in the center and very near it.

He farther obſerves, that the moſt luminous of the priſmatic colours are the yellow and orange, which affect the ſenſe more ſtrongly than all the reſt together; and next to theſe in ſtrength are the red and green; and that the blue, indigo, and violet, compared with theſe, are much darker and fainter, and compared with the other ſtronger colours, little to be regarded; and that therefore the images of the objects are to be placed not in the focus of the mean refrangible rays, which are in the confine of green and blue, but in the middle of the orange and yellow, there where the colour is moſt luminous, that which is in the brighteſt yellow, that yellow which inclines more to orange than to green.

From all theſe conſiderations, and by an elaborate calculation, he infers, that though the whole breadth of the image of a lucid point be $\frac{1}{55}$th of the diameter of the aperture of the object-glaſs, yet the ſenſible image of the ſame is ſcarce broader than a circle whoſe diameter is $\frac{1}{250}$th part of the diameter of the aperture of the object-glaſs of a good teleſcope; and hence he accounts for the apparent diameters of the fixed ſtars as obſerved with teleſcopes by aſtronomers, although in reality they are but points.

The like reaſoning is applicable to the circle of diſſipation on the retina of the human eye; and therefore we may leſſen the angular aberration, before computed at 15′, in the ratio of 250 to 55, which will reduce it to 3′ 18″.

This reduced angle of aberration may perhaps be double the apparent diameter of the brighteſt fixed ſtars to an eye diſpoſed for ſeeing moſt diſtinctly by parallel rays; or, if ſhort-ſighted, aſſiſted by a proper concave lens; which may be thought a ſufficient approximation in an explication grounded on a diſſipation of rays, to which a preciſe limit cannot be aſſigned, on account of the continual increaſe of denſity from the circumference to the center. Certainly ſome ſuch angle of aberration is neceſſary to account for the ſtars appearing under any ſenſible angle to ſuch an eye; and if we were, without reaſon, to ſuppoſe the images on the retina to be perfect, we ſhould be put to a much greater difficulty to account for the fixed ſtars appearing otherwiſe than as points, than we have now been to account for the actual diſtinctneſs of our ſight.

The leſs apparent diameter of the ſmaller fixed ſtars agrees alſo with this theory; for the leſs luminous the circle of diſſipation is, the nearer we muſt look towards its center to find rays ſufficiently denſe to move the ſenſe. From Sir ISAAC NEWTON's geometrical account of the relative denſity of the rays in the circle of diſſipation, given in his ſyſtem of the world, it may be inferred, that the apparent diameters of the fixed ſtars, as depending on this cauſe, are nearly as their whole quantity of light.

In farther elucidation of this ſubject let me add my own experiment. When I look at the brighter fixed ſtars, at conſiderable elevations, through a concave glaſs fitted, as I am ſhort-ſighted, to ſhew them with moſt diſtinctneſs, they appear to me without ſcintillation, and as a ſmall round circle of fire of a ſenſible magnitude. If I look at them without the concave glaſs, or with one not ſuited to my eye, they appear to caſt out rays of a determinate figure, not exactly the ſame in both eyes, ſomewhat like branches of trees (which doubtleſs ariſe from ſomething in the conſtruction of the eye) and to ſcintillate a little, if the air be not very clear. To ſee day objects with moſt diſtinctneſs, I require a leſs concave lens by one degree than for ſeeing the ſtars beſt by night, the cauſe of which ſeems to be, that the bottom of the eye being illuminated by the day objects, and thereby rendered a light ground, obſcures the fainter colours blue indigo and violet in the circle of diſſipation, and therefore the beſt image of the object will be found in the focus of the bright yellow rays, and not in that of the mean refrangible ones, or the dark green, agreeable to NEWTON's remark, and conſequently nearer the retina of a ſhort-ſighted perſon; but the parts of the retina ſurrounding the circle of diſſipation of a ſtar being in the dark, the fainter colours, blue, indigo, and violet, will have ſome ſhare in forming the image, and conſequently the focus will be ſhorter.

The apparent diameter of the ſtars here accounted for is different from that explained by Dr. JURIN, in his Eſſay on diſtinct and indiſtinct viſion, ariſing from the natural conſtitution of the generality of eyes to ſee objects moſt diſtinct at moderate diſtances, and few being capable of altering their conformation enough to ſee diſtant objects, and among them the celeſtial ones, with equal diſtinctneſs. But the cauſe of error, which I have pointed out, will affect all eyes, even thoſe which are adapted to diſtant objects.

If this attempt to ſhew the compatibility of the actual diſtinctneſs of our ſight with the different refrangibility of light ſhall be admitted as juſt and convincing, we ſhall have freſh reaſon to admire the wiſdom of the Creator in ſo adapting the aperture of the pupil and the different refrangibility of light to each other, as to render the picture of objects upon the retina relatively, though not abſolutely, perfect, and fitted for every uſeful purpoſe; "where," to borrow the words of our religious and oratorical philoſopher DERHAM, "all the "glories of the heavens and earth are brought and exquiſitely "pictured."

Nor does it appear, that any material advantage would have been obtained, if the image of objects on the retina had been made abſolutely perfect, unleſs the acuteneſs of the optic nerve ſhould have been increaſed at the ſame time; as the *minimum viſibile* depends no leſs on that circumſtance than the other. But that the ſenſibility of the optic nerve could not have been much increaſed beyond what it is, without great inconvenience to us, may be eaſily conceived, if we only conſider the forcible impreſſion made on our eyes by a bright ſky, or even the day objects illuminated by a ſtrong ſun. Hence we may conclude, that ſuch an alteration would have rendered our ſight painful inſtead of pleaſant, and noxious inſtead of uſeful. We might indeed have been enabled to ſee more in the ſtarry heavens with the naked eye, but it muſt have been at the expence of our daily labours and occupations, the immediate and neceſſary employment of man.

I ſhall only mention farther, and obviate an objection to the diffuſion of the rays upon the retina by the different refrangibility of light. It may be ſaid, that the ocular aberration, being a ſeparate cauſe from any effect of the teleſcope, ſhould ſubſiſt equally when we obſerve a ſtar through a teleſcope as when we look at it with the naked eye; and that therefore the fixed ſtars could not appear ſo ſmall as they have been found to do through the beſt teleſcopes, and particularly by Dr. HERSCHEL, with his excellent ones. To this I anſwer, that the ocular aberration, which is proportional to the diameter of the pupil when we uſe the naked eye, is proportional to the diameter of the pencil of rays at the eye when we look through a teleſcope, which being many times leſs than that of the pupil itſelf, the ocular aberration will be diminiſhed in proportion, and become inſenſible.

XXII. *Experiments and Obſervations on Electricity.* *By Mr.* William Nicholſon; *communicated by Sir* Joſeph Banks, *Bart. P. R. S.*

Read June 25, 1789.

Sect. I. *On the Excitation of Electricity.*

1. A GLASS cylinder was mounted, and a cuſhion applied with a ſilk flap, proceeding from the edge of the cuſhion over its ſurface, and thence half round the cylinder. The cylinder was then excited by applying an amalgamed leather in the uſual manner. The electricity was received by a conductor, and paſſed off in ſparks to Lane's electrometer. By the frequency of theſe ſparks, or by the number of turns required to cauſe ſpontaneous exploſion of a jar, the ſtrength of the excitation was aſcertained.

2. The cuſhion was withdrawn about one inch from the cylinder, and the excitation performed by the ſilk only. A ſtream of fire was ſeen between the cuſhion and the ſilk; and much fewer ſparks paſſed between the balls of the electrometer.

3. A roll of dry ſilk was interpoſed, to prevent the ſtream from paſſing between the cuſhion and the ſilk. Very few ſparks then appeared at the electrometer.

4. A metallic rod, not inſulated, was then interpoſed, inſtead of the roll of ſilk, ſo as not to touch any part of the

apparatus. A denſe ſtream of electricity appeared between the rod and the ſilk, and the conductor gave very many ſparks.

5. The knob of a jar being ſubſtituted in the place of the metallic rod, it became charged negatively.

6. The ſilk alone, with a piece of tin-foil applied behind it, afforded much electricity, though leſs than when the cuſhion was applied with a light preſſure. The hand, being applied to the ſilk as a cuſhion, produced a degree of excitation ſeldom equalled by any other cuſhion.

7. The edge of the hand anſwered as well as the palm.

8. When the excitation by a cuſhion was weak, a line of light appeared at the anterior part of the cuſhion, and the ſilk was ſtrongly diſpoſed to receive electricity from any uninſulated conductor. Theſe appearances did not obtain when the excitation was by any means made very ſtrong.

9. A thick ſilk, or two or more folds of ſilk, excited worſe than a ſingle very thin flap. I uſe the ſilk which the milleners call Perſian.

10. When the ſilk was ſeparated from the cylinder, ſparks paſſed between them; the ſilk was found to be in a weak negative, and the cylinder in a poſitive ſtate.

The foregoing experiments ſhew that the office of the ſilk is not merely to prevent the return of electricity from the cylinder to the cuſhion, but that it is the chief agent in the excitation; while the cuſhion ſerves only to ſupply the electricity, and perhaps increaſe the preſſure at the entering part. There likewiſe ſeems to be little reaſon to doubt but that the diſpoſition of the electricity to eſcape from the ſurface of the cylinder is not prevented by the interpoſition of the ſilk, but by a compenſation after the manner of a charge; the ſilk being then as ſtrongly negative as the cylinder is poſitive: and, laſtly, that the line of light between the ſilk and cuſhion in weak excitations does not conſiſt of returning electricity, but of electricity which paſſes to the cylinder, in conſequence of its not having been ſufficiently ſupplied, during its contact with the rubbing ſurface.

11. When the excitation was very ſtrong in a cylinder newly mounted, flaſhes of light were ſeen to fly acroſs its inſide, from the receiving ſurface to the ſurface in contact with the cuſhion, as indicated by the bruſh figure. Theſe made the cylinder ring as if ſtruck with a bundle of ſmall twigs. They ſeem to have ariſen from part of the electricity of the cylinder taking the form of a charge. This appearance was obſerved in a nine-inch and a twelve-inch cylinder, and the property went off in a few weeks. Whence it appears to have been chiefly occaſioned by the rarity of the internal air produced by handling, and probably reſtored by gradual leaking of the cement.

12. With a view to determine what happens in the inſide of the cylinder, recourſe was had to a plate machine. One cuſhion was applied with its ſilken flap. The plate was nine inches in diameter and two-tenths of an inch thick. During the excitation, the ſurface oppoſite the cuſhion ſtrongly attracted electricity, which it gave out when it arrived oppoſite the extremity of the flap. So that a continual ſtream of electricity paſſed through an inſulated metallic bow terminating in balls, which were oppoſed, the one to the ſurface oppoſite the extremity of the ſilk, and the other oppoſite the cuſhion; the former ball ſhewing poſitive, and the latter negative ſigns. The knobs of two jars being ſubſtituted in the place of theſe balls, the jar, applied to the ſurface oppoſed to the cuſhion, was charged negatively, and the other poſitively. This diſpoſition of the back ſurface ſeemed, by a few trials, to be weaker the

ſtronger the action of the cuſhion, as judged by the electricity on the cuſhion ſide.

Hence it follows, that the internal ſurface of a cylinder is o far from being diſpoſed to give out electricity during the friction by which the external ſurface acquires it, that it even greedily attracts it.

13. A plate of glaſs was applied to the revolving plate, and thruſt under the cuſhion in ſuch a manner as to ſupply the place of the ſilk flap. It rendered the electricity ſtronger, and appears to be an improvement of the plate machine; to be admitted if there were not eſſential objections againſt the machine itſelf.

14. Two cuſhions were then applied on the oppoſite ſurfaces with their ſilk flaps, ſo as to claſp the plate between them. The electricity was received from both by applying the finger and thumb to the oppoſite ſurfaces of the plate. When the finger was advanced a little towards its correſpondent cuſhion, ſo that its diſtance was leſs than between the thumb and its cuſhion, the finger received ſtrong electricity, and the thumb none; and, contrariwiſe, if the thumb were advanced beyond the finger, it received all the electricity, and none paſſed to the finger. This electricity was not ſtronger than was produced by the good action of one cuſhion applied ſingly.

15. The cuſhion in experiment 12. gave moſt electricity when the back ſurface was ſupplied, provided that ſurface was ſuffered to retain its electricity till the rubbed ſurface had given out its electricity.

From the two laſt paragraphs it appears, that no advantage is gained by rubbing both ſurfaces; but that a well managed friction on one ſurface will accumulate as much electricity as the preſent methods of excitation ſeem capable of collecting;

but that when the excitation is weak, on account of the electric matter not paſſing with ſufficient facility to the rubbed ſurface, the friction enables the oppoſite ſurface to attract or receive it, and if it be ſupplied, both ſurfaces will paſs off in the poſitive ſtate; and either ſurface will give out more electricity than is really induced upon it, becauſe the electricity of the oppoſite ſurface forms a charge. It may be neceſſary to obſerve, that I am ſpeaking of the facts or effects produced by friction; but how the rubbing ſurfaces act upon each other to produce them, whether by attraction, or otherwiſe, I do not here enquire.

It will hereafter be ſeen, that plate machines do not collect more electricity than cylinders (in the hands of the electrical operators of this metropolis) do with half the rubbed ſurface; which is a corroboration of the inference here made.

16. When a cylinder is weakly excited, the appearances mentioned (par. 8.) are more evident, the more rapid the turning. In this caſe, the avidity of the ſurface of the cylinder beneath the ſilk is partly ſupplied from the edge of the ſilk which throws back a broad caſcade of fire, ſometimes to the diſtance of above twelve inches. From theſe cauſes it is that there is a determinate velocity of turning required to produce the maximum of intenſity in the conductor. The ſtronger the excitation the quicker may be the velocity; but it rarely exceeds five feet of the glaſs to paſs the cuſhion in a ſecond.

17. If a piece of ſilk be applied to a cylinder, by drawing down the ends, ſo that it may touch half the circumference, and the cylinder be then turned and excited by applying the amalgamed leather, it will become very greedy of electricity during the time it paſſes under the ſilk. And if the entering ſurface of the glaſs be ſupplied with electricity, it will give it out at the other extremity of contact; that is to ſay, if inſu-

lated conductors be applied at the touching ends of the ſilk, the one will give, and the other receive, electricity until the intenſities of their oppoſite ſtates are as high as the power of the apparatus can bring them; and theſe ſtates will be inſtantly reverſed by turning the cylinder in the oppoſite direction.

As this diſcovery promiſes to be of the greateſt uſe in electrical experiments, becauſe it affords the means of producing either the plus or minus ſtates in one and the ſame conductor, and of inſtantly repeating experiments with either power, and without any change of poſition or adjuſtment of the apparatus, it evidently deſerved the moſt minute examination.

18. There was little hope (par. 6.) that cuſhions could be diſpenſed with. They were therefore added; and it was then ſeen, that the electrified conductors were ſupplied by the difference between the action of the cuſhion which had the advantage of the ſilk and that which had not; ſo that the naked face of the cylinder was always in a ſtrong electric ſtate. Methods were uſed for taking off the preſſure of the receiving cuſhion; but the extremity of the ſilk, by the conſtruction, not being immediately under that cuſhion, gave out large flaſhes of electricity with the power that was uſed. Neither did it appear practicable to preſent a row of points or other apparatus to intercept the electricity which flew round the cylinder; becauſe ſuch an addition would have materially diminiſhed the intenſity of the conductor, which in the uſual way was ſuch as to flaſh into the air from rounded extremities of four inches diameter, and made an inch and half ball become luminous and blow like a point. But the greateſt inconvenience was, that the two ſtates with the backward and forward turn were ſeldom equal; becauſe the diſpoſition of the amal-

gam on the ſilk, produced by applying the leather to the cylinder in one direction of turning, was the reverſe of what muſt take place when the contrary operation was performed.

Notwithſtanding all this, as the intenſity with the two cuſhions was ſuch as moſt operators would have called ſtrong, the method may be of uſe, and I ſtill mean to make more experiments when I get poſſeſſion of a very large machine which is now in hand.

19. The more immediate advantage of this diſcovery is, that it ſuggeſted the idea of two fixed cuſhions with a moveable ſilk flap and rubber. Upon this principle, which is ſo ſimple and obvious, that it is wonderful it ſhould have been ſo long overlooked, I have conſtructed a machine with one conductor, in which the two oppoſite and equal ſtates are produced by the ſimple proceſs of looſening the leather rubber, and letting it paſs round with the cylinder (to which it adheres) until it arrives at the oppoſite ſide, where it is again faſtened. A wiſh to avoid prolixity prevents my deſcribing the mechaniſm by which it is let go, and faſtened in an inſtant, at the ſame time that the cuſhion is made either to preſs or is withdrawn, as occaſion requires.

20. Although the foregoing ſeries of experiments naturally lead us to conſider the ſilk as the chief agent in excitation; yet as this buſineſs was originally performed by a cuſhion only, it becomes an object of enquiry to determine what happens in this caſe.

21. The great Beccaria * inferred, that in a ſimple cuſhion, the line of fire, which is ſeen at the extremity of contact from which the ſurface of the glaſs recedes, conſiſts of returning electricity; and Dr. Nooth grounded his happy

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* Philoſophical Tranſactions, Vol. LVI. p. 117.

invention of the ſilk flap upon the ſame ſuppoſition. The former aſſerts, that the lines of light both at the entering and departing parts of the ſurface are abſolutely ſimilar; and thence infers, that the cuſhion receives on the one ſide, as it certainly does on the other. I find, however, that the fact is directly contrary to this aſſertion; and that the oppoſite inference ought to be made, as far as this indication can be reckoned concluſive: for the entering ſurface exhibits many luminous perpendiculars to the cuſhion, and the departing ſurface exhibits a neat uniform line of light. This circumſtance, together with the conſideration that the line of light behind the ſilk in par. 8. could not conſiſt of returning electricity, ſhewed the neceſſity of farther examination. I therefore applied the edge of the hand as a rubber, and by occaſionally bringing forward the palm, I varied the quantity of electricity which paſſed near the departing ſurface. When this was the greateſt, the ſparks at the electrometer were the moſt numerous. But, as the experiment was liable to the objection that the rubbing ſurface was variable, I paſted a piece of leather upon a thin flat piece of wood, then amalgamed its whole ſurface, and cut its extremity off in a neat right line cloſe to the wood. This being applied by the conſtant action of a ſpring againſt the cylinder, produced a weak excitation, and the line where the contact of the cylinder and leather ceaſed (as abruptly as poſſible) exhibited a very narrow fringe of light. Another piece of wood was prepared of the ſame width as the rubber, but one quarter of an inch thick, with its edges rounded, and its whole ſurface covered with tin-foil. This was laid on the back of the rubber, and was there held by a ſmall ſpring, in ſuch a manner as that it could be ſlided onward, ſo as occaſionally to project beyond the rubber, and cover the departing and excited ſurface of the cylinder, without touching it. The ſparks at the electrometer were four times as numerous when this metallic piece was thus projected; but no electricity was obſerved to paſs between it and the cylinder. The metallic piece was then held in the hand to regulate its diſtance from the glaſs; and it was found, that the ſparks at the electrometer increaſed in number as it was brought nearer, until light appeared between the metal and the cylinder, at which time they became fewer the nearer it was brought, and at laſt ceaſed when it was in contact.

The following concluſions appear to be deducible from theſe experiments. 1. The line of light on a cylinder departing from a ſimple cuſhion conſiſts of returning electricity; 2. the projecting part of the cuſhion compenſates the electricity upon the cylinder, and by diminiſhing its intenſity prevents its ſtriking back in ſuch large quantities as it would otherwiſe do; 3. that if there were no ſuch compenſation, very little of the excited electricity would be carried off; and, 4. that the compenſation is diminiſhed, or the intenſity increaſed, in an higher ratio than that of the diſtance of the compenſating ſubſtance; becauſe if it were not, the electricity which has been carried off from an indefinitely ſmall diſtance, would never fly back from a greater diſtance and form the edge of light.

22. I hope the conſiderable intenſity I ſhall ſpeak of will be an apology for deſcribing the manner in which I produce it. I wiſh the theory of this very obſcure proceſs were better known; but no conjecture of mine is worth mentioning. The method is as follows:

Clean the cylinder, and wipe the ſilk.

Greaſe the cylinder by turning it againſt a greaſed leather till it is uniformly obſcured. I uſe the tallow of a candle.

Turn the cylinder till the ſilk flap has wiped off ſo much of the greaſe as to render it ſemi-tranſparent.

Put ſome amalgam on a piece of leather, and ſpread it well ſo that it may be uniformly bright. Apply this againſt the turning cylinder. The friction will immediately increaſe, and the leather muſt not be removed until it ceaſes to become greater.

Remove the leather, and the action of the machine will be very ſtrong.

My rubber, as before obſerved, conſiſts of the ſilk flap paſted to a leather, and the cuſhion is preſſed againſt the ſilk by a ſlender ſpiral ſpring in the middle of its back. The cuſhion is looſely retained in a groove, and reſts againſt the ſpring only, in ſuch a manner that by a ſort of libration upon it as a fulcrum, it adapts itſelf to all the irregularities of the cylinder, and never fails to touch in its whole length. There is no adjuſtment to vary the preſſure, becauſe the preſſure cannot be too ſmall when the excitation is properly made. Indeed, the actual withdrawing of the cuſhion to the diſtance of one-tenth of an inch from the ſilk, as in par. 2. will not materially affect a good excitation.

The amalgam is that of Dr. HIGGINS, compoſed of zinc and mercury. If a little mercury be added to melted zinc, it renders it eaſily pulverable, and more mercury may be added to the powder to make a very ſoft amalgam. It is apt to cryſtallize by repoſe, which ſeems in ſome meaſure to be prevented by triturating it with a ſmall proportion of greaſe: and it is always of advantage to triturate it before uſing.

A very ſtrong excitation may be produced by applying the amalgamed leather to a clean cylinder with a clean ſilk. But it ſoon goes off, and is not ſo ſtrong as the foregoing, which laſts ſeveral days.

23. To give ſome diſtinctive criterions by which other electricians may determine whether the intenſity they produce exceeds or falls ſhort of that which this method affords, I ſhall mention a few facts.

With a cylinder 7 inches diameter and cuſhion 8 inches long, three bruſhes at a time conſtantly flew out of a three-inch ball in a ſucceſſion too quick to be counted, and a ball of 1½ inch diameter was rendered luminous, and produced a ſtrong wind like a point. A nine-inch cylinder with an eight-inch cuſhion occaſioned frequent flaſhes from the round end of a conductor 4 inches diameter: with a ball of 2½ inches diameter the flaſhes ceaſed now and then, and it began to appear luminous: a ball of 1½ inch diameter firſt gave the uſual flaſhes; then, by quicker turning, it became luminous with a bright ſpeck moving about on its ſurface, while a conſtant ſtream of air ruſhed from it; and, laſtly, when the intenſity was greateſt, bruſhes, of a different kind from the former, appeared. Theſe were leſs luminous, but better defined in the branches; many ſtarted out at once with a hoarſe ſound. They were reddiſh at the ſtem, ſooner divided, and were greeniſh at the point next the ball, which was braſs. A ball of $\frac{4}{10}$ inch diameter was ſurrounded by a ſteady faint light, enveloping its exterior hemiſphere, and ſometimes a flaſh ſtruck out at top. When the excitation was ſtrongeſt a few flaſhes ſtruck out ſideways. The horizontal diameter of the light was longeſt, and might meaſure one inch, the ſtem of the ball being vertical.

This laſt phænomenon is ſimilar to a natural event related by M. LOAMMI BALDWIN *, who raiſed an electrical kite in

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* Memoirs of the American Academy, Vol. I. p. 257.

July, 1771, during the approach of a ſevere thunder-ſtorm, and obſerved himſelf to be ſurrounded by a rare medium of fire, which, as the cloud roſe nearer the zenith, and the kite roſe higher, continued to extend itſelf with ſome gentle faint flaſhes. Mr. BALDWIN felt no other effect than a general weakneſs in his joints and limbs, and a kind of liſtleſs feeling; all which he obſerves might poſſibly be the effect of ſurprize, though it was ſufficient to diſcourage him from perſiſting in any farther attempt at that time. He therefore drew in his kite, and retired to a ſhop till the ſtorm was over, and then went to his houſe, where he found his parents and friends much more ſurpriſed than he had been himſelf; who, after expreſſing their aſtoniſhment, informed him, that he appeared to them (during the time he was raiſing the kite) to be in the midſt of a large bright flame of fire, attended with flaſhings; and that they expected every moment to ſee him fall a ſacrifice to the flame. The ſame was obſerved by ſome of his neighbours, who lived near the place where he ſtood.

This fact is ſimilar to another obſerved by M. DE SAUSSURE on the Alps, and both are referable to my luminous ball with the ſecond kind of bruſh. The cloud muſt have been negative.

With a 12-inch cylinder, and rubber of 7½ inches, a five-inch ball gave frequent flaſhes, upwards of 14 inches long, and ſometimes a ſix-inch ball would flaſh. I do not mention the long ſpark, becauſe I was not provided with a favourable apparatus for the two larger cylinders. The 7-inch cylinder affords a ſpark of 10¾ inches at beſt. The 9-inch cylinder, not having its conductor inſulated on a ſupport ſufficiently high, afforded flaſhes to the table which was 14 inches diſtant. And the 12-inch cylinder, being mounted only as a model or trial for conſtructing a larger apparatus, is defective in ſeveral reſpects which I have not thought fit to alter. When the five-inch ball gives flaſhes, the cylinder is enveloped on all ſides with fire which ruſhes from the receiving part of the conductor. I never uſe points, but in a ſimple machine bring the conductor almoſt in contact with the cylinder. In this apparatus that cuſhion to which the rubber is not applied ſerves that purpoſe.

24. Theſe marks exhibit the intenſity as deduced from ſimple electrifying. I will now mention the rate of charging, which was nearly the ſame in all the three cylinders.

A large jar of 350 ſquare inches, or near 2½ ſquare feet, with an uncoated varniſhed rim, of more than four inches in height, was made to explode ſpontaneouſly over the rim. The jar, when broken, proved to be 0.082 inches thick on an average; and the number of ſquare feet of the ſurface of the cylinder which was rubbed, to produce the charge of one foot, was, when leaſt, 18.03, and when moſt, with good excitation, 19.34. The great machine at Harlem charges * a ſingle jar of one foot ſquare by the friction of 66.6 ſquare feet, and charges its battery of 225 ſquare feet at the rate of 94.8 ſquare feet rubbed for each foot. The intenſity of electricity on the ſurface of the glaſs is therefore conſiderably leſs than one-fourth of that here ſpoken of; but if we take the moſt favourable number 66.6 at the commencement of turning, and halve it on account of the unavoidable imperfection of a plate machine (as ſhewn in par. 14.), it will be found, that the management applied to that machine would cauſe a cylinder to charge one ſquare foot by the friction of 33⅓ ſquare feet. It muſt be obſerved, however, that M. VAN MARUM's own machine, con-

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* To explode from the central wire, which, from ſome trials, I find to require leſs force than from coating to coating at equal diſtances.

ſiſting of two plates, 33 inches diameter, has only half the intenſity, though he reckons it a very good one. This machine is about equal in abſolute power to my 9-inch cylinder, with its ſhort rubber; but it is near thirty times as dear in price. In all theſe deductions I omit the computations, for the ſake of brevity, and becauſe they are eaſily made. The *data* are found in the deſcription of the Teylerian machine, and its continuation publiſhed at Harlem in the years 1785 and 1787.

I ſhall here take the liberty of obſerving, that the action of the cylinder, by a ſimple cuſhion or the hand, which excited the aſtoniſhment of all Europe, in the memory of our co-temporaries, was firſt improved by the addition of a leathern flap; then by moiſtening the rubber; afterwards by applying the amalgam; and, laſtly, by the addition of a ſilk flap. Now, I find, by experiment, that we at preſent obtain upwards of forty times the intenſity which the bare hand produces; and conſequently that, ſince eighteen times our preſent intenſity will equal the utmoſt we can now condenſe on ſtrong glaſs even in the form of a charge; we have a leſs ſtep to take before we arrive at that amazing power, than our immediate predeceſſors have already made.

My 9-inch cylinder, when broken, proved to be $\frac{1}{11}$ of an inch thick.

SECT. II. *Upon the luminous Appearances of Electricity and the Action of Points.*

25. Some of the luminous appearances, with balls in the poſitive ſtate, have been ſlightly noticed as criterions of intenſity. I ſhall here add, that the eſcape of negative electricity from a ball is attended with the appearance of ſtrait ſharp ſparks with a

hoarſe or chirping noiſe. When the ball was leſs than two inches in diameter, it was uſually covered with ſhort flames of this kind, which were very numerous.

26. When two equal balls were preſented to each other, and one of them was rendered ſtrongly poſitive, while the other remained in connection with the earth, the poſitive bruſh or ramified ſpark was ſeen to paſs from the electrified ball: when the other ball was electrified negatively, and the ball, which before had been poſitive, was connected with the ground, the electricity (paſſing the ſame way according to FRANKLIN) exhibited the negative flame, or denſe ſtraight and more luminous ſpark, from the negative ball; and when the one ball was electrified *plus* and the other *minus*, the ſigns of both electricities appeared. If the interval was not too great, the long zig-zag ſpark of the *plus* ball ſtruck to the ſtrait flame of the *minus* ball, uſually at the diſtance of about one-third of the length of the latter from its point, rendering the other two-thirds very bright. Sometimes, however, the poſitive ſpark ſtruck the ball at a diſtance from the negative flame. Theſe effects are repreſented in Plate IV. fig. 1, 2 and 3.

27. Two conductors of three-quarters of an inch diameter, with ſpherical ends of the ſame diameter, were laid parallel to each other, at the diſtance of about two inches, in ſuch a manner as that the ends pointed in oppoſite directions, and were ſix or eight inches aſunder. Theſe, which may be diſtinguiſhed by the letters P and M, were ſucceſſively electrified as the balls were in the laſt paragraph. When one conductor P was poſitive, fig. 5. it exhibited the ſpark of that electricity at its extremity, and ſtruck the ſide of the other conductor M. When the laſt-mentioned conductor M was electrified negatively, fig. 4. the former being in its turn connected with the earth,

the ſparks ceaſed to ſtrike as before, and the extremity of the electrified conductor M exhibited negative ſigns, and ſtruck the ſide of the other conductor. And when one conductor was electrified *plus* and the other *minus*, fig. 6. both ſigns appeared at the ſame time, and continual ſtreams of electricity paſſed between the extremities of each conductor to the ſide of the other conductor oppoſed to it. In each of theſe three caſes, the current of electricity, on the hypotheſis of a ſingle fluid, paſſed the ſame way.

28. In drawing the long ſpark from a ball of four inches diameter, I found it of ſome conſequence that the ſtem ſhould not be too ſhort, becauſe the vicinity of the large prime conductor altered the diſpoſition of the electricity to eſcape; I therefore made a ſet of experiments, the reſult of which ſhewed, that the diſpoſition of balls to receive or emit electricity is greateſt when they ſtand remote from other ſurfaces in the ſame ſtate; and that between this greateſt diſpoſition in any ball, whatever may be its diameter, every poſſible leſs degree may be obtained by withdrawing the ball towards the broader or leſs convex ſurface out of which its ſtem projects, until at length the ball, being wholly depreſſed beneath that ſurface, loſes the diſpoſition entirely. From theſe experiments it follows, that a variety of balls is unneceſſary in electricity; becauſe any ſmall ball, if near the prime conductor, will be equivalent to a larger ball whoſe ſtem is longer.

29. From comparing ſome experiments, made by myſelf many years ago, with the preſent ſet, I conſidered a point as a ball of an indefinitely ſmall diameter, and conſtructed an inſtrument conſiſting of a braſs ball of ſix inches diameter, through the axis of which a ſtem, carrying a fine point, was ſcrewed. When this ſtem is fixed in the prime conductor, if

the ball be moved on its axis in either direction, it cauſes the fine point either to protrude through a ſmall hole in its external ſurface, or to withdraw itſelf; becauſe by this means the ball runs along the ſtem. The diſpoſition of the point to tranſmit electricity may thus be made equal to that of any ball whatever, from the minuteſt ſize to the diameter of ſix inches. See fig. 7. let. A.

30. The action of pointed bodies has been a ſubject of diſcuſſion ever ſince it was firſt diſcovered, and is not yet well explained. To thoſe who aſcribe this effect to the figure of electric atmoſpheres, and their diſpoſition to fly off, it may be anſwered, that they ought firſt to prove their exiſtence, and then ſhew why the cauſe which accumulated them does not prevent their eſcape; not to mention the difficulty of explaining the nature of negative atmoſpheres. If theſe be ſuppoſed to conſiſt of electrified air, it will not be eaſy to ſhew why a current of air paſſing near a prime conductor does not deſtroy its effects. The opinion, ſupported by the celebrated VOLTA and others, that a point is the coating to an infinitely ſmall plate of air, does not appear better founded: for ſuch a plate muſt be broken through at a greater diſtance only becauſe higher charged; whence it would follow, that points ſhould not act but at high intenſities. I muſt likewiſe take notice, as a proof that the charge has little to do here, that if a ball be preſented to the prime conductor, at the ſame time that a point proceeds from the oppoſite ſide of the ball, the electricity will paſs by the point, though it is obliged to go round the ball for that purpoſe; but it can hardly be doubted, that whatever charge obtains in this caſe is on the ſurface of the ball next the conductor, and not on the remote ſide to which the electricity directs its courſe.

31. ACHARD's experiments with a number of pointed cones, ſcrewed in a plate of metal, and likewiſe the pointed apparatus deſcribed (par. 29) ſhew that the effect of points depends on the remoteneſs of their extremities from the other parts of the conductor. This leads to the following general law.

In any electrified conductor the tranſition or eſcape of electricity will be made chiefly from that part of the ſurface which is the moſt remote from the natural ſtate.

Thus in the apparatus of the ball and ſtem, the point having a communication with the reſt of the whole conductor, conſtantly poſſeſſes the ſame intenſity; but the influence of the ſurrounding ſurface of the ball diminiſhes its capacity. This diminution is leſs the farther the ball is withdrawn, and conſequently the point will really poſſeſs more electricity, and be more diſpoſed to give it out when it is prominent than when depreſſed. The ſame explanation ſerves for negative electricity.

32. The effect of a poſitive ſurface appears to extend farther than that of a negative: for the point acts like a ball when conſiderably more prominent if it be poſitive than it will if negative. This property was uſed by me ſome years ago for the conſtruction of an inſtrument to diſtinguiſh the two electricities [*].

For the ſake of conciſeneſs I paſs over many facts which have preſented themſelves in the courſe of my experiments on the two electricities, and content myſelf with obſerving, that there is ſcarcely any experiment made with the poſitive power which will not afford a reſult worthy of notice, if repeated with the negative.

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[*] Introduction to Natural Philoſophy, Vol. II. p. 320.

33. When we conſider that our machines can cauſe a ball of an inch and half diameter to act like a point, and that our apparatus makes a point act like a ball; if at the ſame time we remark the ſmall elevation of our conductors for lightning above the extended ſurface of the ground, and the ſmall ſize of the balls propoſed by ſome to be uſed as terminations; the diſpute, which was ſo much agitated reſpecting them, will perhaps be found to relate to a very minute circumſtance, among the many which govern the great operations of nature. It does not ſeem probable, that any conductor would act ſilently if the main courſe of the electricity of a negative cloud were to paſs through it, and many would probably receive the ſtroke from a poſitive cloud. It does not, however, follow from this, that they might not conduct it with ſafety.

SECT. III. *Of compenſated Electricity.*

34. It is unneceſſary to inſiſt upon what is called the equilibrium of an electrical charge, becauſe Dr. FRANKLIN has admirably explained it according to his hypotheſis. But there is another important particular, which has been almoſt entirely overlooked, namely, the uncompenſated electricity which is as eſſential to the charge as that which is in equilibrio. Whenever a jar is charged, the greateſt part of the electricity becomes latent on account of the compenſation; but there is a certain proportion which remains on the inſulated ſide, and exerts its force to prevent the electricity from returning to the outer ſurface. In moderate intenſities, this will explode, and carry the charge with it, to diſtances which are in proportion to the quantity of the charge itſelf; but in greater intenſities the diſtances greatly exceed that proportion. With glaſſes of different thickneſſes, this intenſity,

as meaſured by the exploſive ſpark, is as the thickneſs, when the charges are equal, as Mr. CAVENDISH has determined, and I find likewiſe by experiments with thin ſubſtances; but when the thickneſſes are greater, it increaſes in a higher proportion, as is found by the exploſion which takes place between the electrophore and its plate, as well as by other experiments.

35. This uncompenſated part of the charge (which is commonly in proportion to the quantity of latent or compenſated electricity, or to the diſtance at which it exerts its action) was found to be greatly increaſed when a ſeries of jars were made to charge each other. If a jar be inſulated and made to explode by LANE's electrometer at a determinate number of turns; and another jar be then connected with its external coating ſo as to become charged by that means, the exploſion, from the outſide of the laſt to the inſide of the firſt, will take place at the electrometer (unaltered) with much fewer turns. Or if the electrometer be altered till the exploſion takes place at the original number, the diſtance will be much greater than before. Hence we ſee, that the intenſity of the uncompenſated part muſt be greater when there is a greater charge to be maintained, whether it be on one ſurface only, or on two ſurfaces ſucceſſively connected. I have not yet made the experiments neceſſary to aſcertain the law of this laſt action.

36. It is evident, that the breaking of jars is not effected by any attraction between the electricities which form the charge, but by this neceſſary ſurplus: for thicker glaſſes require much leſs electricity to produce an intenſity which breaks them than thinner do; and I found a piece of Muſcovy talc, one hundredth of an inch thick, to bear a charge conſiſting of ten times the quantity of electricity which was ſufficient to have charged an equal ſurface of common glaſs ſo as to break it.

But the intenſity of the very denſe charge on the talc was ſo low as to afford an exploſion of no more than about one-tenth of an inch, while that of the glaſs jar it was compared with exploded through about five inches.

The perforation of glaſs by the long ſpark, or by the ſpark through oil or cement, ſeems to depend on the very great intenſity of the electricity which has not time to diffuſe itſelf, but charges a minute part of the ſurface very high.

37. Muſcovy talc * being a very perfect non-conductor, and capable of being divided into plates of leſs thickneſs than one two-hundredth part of an inch, I made many experiments with it, which are too numerous to enter into this Paper. In conſequence of its great capacity it gives very ſtrong ſhocks. Contrary to the aſſertion of BECCARIA, I found that its laminæ are naturally in ſtrong oppoſite ſtates of electricity, and flaſh to each other when torn aſunder in the dark. A large piece being ſplit in two, the parts were found to be in oppoſite ſtates. The greateſt care was taken in theſe experiments to avoid friction, and to uſe ſuch pieces as had never been excited, nor brought near the machine.

38. The moſt plauſible objection againſt the probability of danger from the returning ſtroke of the Earl of STANHOPE is, that the quantity of electricity in an animal is too ſmall to produce any miſchievous effect. This the noble author has anſwered by remarking, that the quantity has not been ſhewn to be ſmall †. My experiments with talc ſhewing that it naturally poſſeſſes much electricity, led me to inveſtigate the quantity which a man may contain. I melted ſealing wax upon BENNET's electrometer with a burning glaſs, and found it pro-

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* I am not certain whether HENLEY or BECCARIA firſt uſed this ſubſtance; but little attention was paid to it by either. † Phil. Tranſ. Vol. LXXVII. p. 143.

duced no electricity either in heating or cooling. I alſo placed a piece of red-hot glaſs upon the ſame inſtrument, and it cooled without affording electric ſigns. Theſe experiments ſhewed, that the natural quantity of electricity is the ſame in theſe bodies, whether they be in the conducting or non-conducting ſtate; and conſequently, if it can be proved, that an electric contains a large quantity of electricity, the inference may be fairly extended to non-electrics. And it will not be diſputed, upon any hypotheſis, but that a non-conductor, or its coating, contains as much of what we call electricity as can be driven out of it in the act of charging. Two ſquare inches of talc, of the thickneſs of 0,011 inch, were repeatedly charged and made to explode over the uncoated part, by each turn of a ſeven-inch cylinder. The intenſity of the excitation was ſuch, that a conductor, of three feet long, and ſeven inches diameter, gave a denſe ſpark of 9 inches long at each turn. Now, in round numbers 45 ſuch plates of talc, laid upon each other, would have formed a ſolid inch of matter; and from this, if fitted up as a BECCARIA's battery, we could with our machine drive out electricity enough ſimply to charge a conductor 45 times as long (neglecting the ends); that is to ſay, we find that one ſolid inch of talc contains electricity enough to charge a conductor of 7 inches diameter, and 135 feet long, ſo high as to give a nine-inch ſpark at leaſt, but how much more it contains we know not.

If it be here objected, that the talc does nothing more than ſeparate the coatings, we may make uſe of gold leaf for our coating; which ſubſtance being (as I find by weight and meaſurement) no more than $\frac{1}{282000}$ of an inch thick, would increaſe the reſult near three thouſand times.

Without referring to the intenſe electricity of a cloud, or the bulk of a man, it may be obſerved, that ſuch a ſpark would be very painful. But to purſue our computation. The cylinder charged a ſquare foot of glaſs, of about 0.08 thick, in 15 turns ſo as to explode over a rim above four inches high. Fifteen of the pieces of talc would therefore poſſeſs as much electricity as makes the charge of a jar of one foot ſquare, and the 45 pieces or ſolid inch would contain enough to charge three ſquare feet. If we ſuppoſe the bulk of a man to be only 3 ſolid feet or 5184 ſolid inches, the natural electricity of this maſs, as deduced from the foregoing facts, will be equal to the charge of a battery of upwards of 15,000 ſquare feet.

I beg leave to obſerve, in concluding this Paper, that I have been very careful in repeating the experiments with many precautions which the experienced in this branch of natural philoſophy will perceive the neceſſity of; though, in order to keep this communication within proper limits, I have here avoided a minute deſcription of them. With the ſame view I have likewiſe forborn to ſpeak either of theory, or of a number of other experimental reſearches I have made during the courſe of this enquiry. Dates are entirely omitted from a conviction that the priority of accidental diſcovery is not worth contending for, and that no diſputes ever ariſe about that general tenor of conduct in the cultivation of ſcience, upon which the rational part of mankind ground their approbation or cenſure.

New North-ſtreet,
May 14, 1789.

P. S. Since the above was written, the *Journal de Phyſique* for April, 1789, has arrived. It contains an excellent Paper of M. VAN MARUM, giving an account of ſome very conſiderable amendments of the rubber, and of the manner of applying it to plate machines. The chief improvement conſiſts in fixing the ſilk to the poſterior part of the rubber, ſo that it covers the whole face, and has the amalgam applied upon it. I cannot, however, avoid expreſſing my ſurprize, that this improvement, which has been in common uſe in England for upwards of twelve years paſt, ſhould now be offered as a diſcovery by ſo experienced a philoſopher. With his new rubbers M. VAN MARUM excites his plates of 33 inches diameter ſo ſtrongly as to produce nearly two-thirds of the former effect of the Teylerian machine, though the rubbed ſurfaces of theſe machines are now as 691 to 2409. This power would charge the ſingle jar by the friction of 28.6 ſquare feet, or the battery by rubbing 36.2 feet, inſtead of the numbers 66.6 and 94.8, as given in par. 24. This is a vaſt acquiſition of intenſity; but ſtill little more than half that of the ſurface of a cylinder, as mentioned in the ſame paragraph. But if par. 14. be admitted to prove that plate machines gain nothing by the friction of the back ſurfaces, it will follow, that M. VAN MARUM's management, if applied to a cylinder, would do better than mine.

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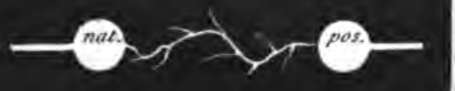
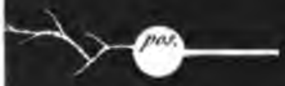


Fig. 2.



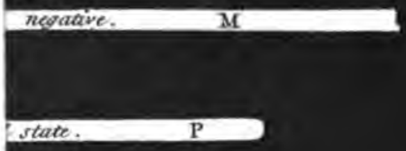
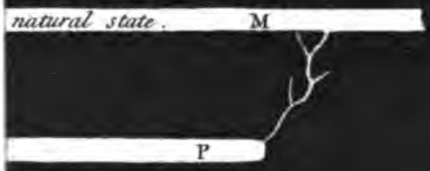


Fig. 4.



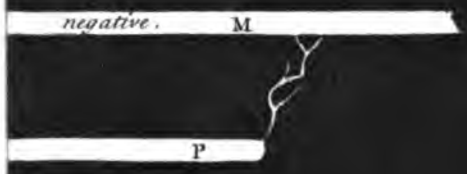


Fig. 6.

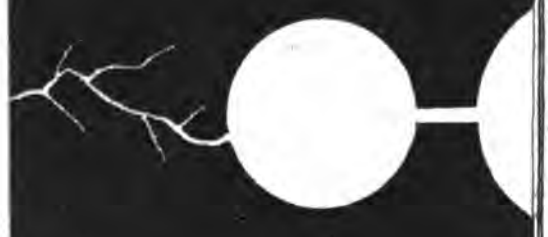


Fig. 7.

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XXIII. *Experiments on the Tranſmiſſion of the Vapour of Acids through an hot earthen Tube, and further Obſervations relating to Phlogiſton. By the Rev.* Joſeph Prieſtley, *LL.D. F.R.S.*

Read July 2, 1789.

IN my late experiments on the *phlogiſtication of ſpirit of nitre by heat* it appeared, that when pure air was expelled from what I called dephlogiſticated ſpirit of nitre, the remainder was left phlogiſticated. This I find abundantly confirmed by repeating the experiments in a different manner, and on a larger ſcale; and I have applied the ſame proceſs to other acids and liquors of a different kind. From theſe it will appear, that oil of vitriol and ſpirit of nitre, in their moſt dephlogiſticated ſtate, conſiſt of a proper ſaturation of the acids with phlogiſton, ſo that what we have called the *phlogiſtication* of them, ought rather to have been called their *ſuper-phlogiſtication.*

I began with treating a quantity of oil of vitriol as I had done the ſpirit of nitre, *viz.* expoſing it to heat in a glaſs tube, hermetically ſealed, and nearly exhauſted; and the reſult was ſimilar to that of the experiment with the nitrous acid, with reſpect to the expulſion of air from it, though the phlogiſtication not appearing by any change of *colour*, I did not in this method aſcertain that circumſtance. The particulars were as follows.

After the acid had been made to boil ſome time, a denſe white vapour appeared in quick motion at a diſtance above the acid, and though, on withdrawing the fire, that vapour diſappeared, it inſtantly re-appeared on renewing the heat. When the tube was cool, I opened it under water, and a quantity of air ruſhed out, though the acid had been made to boil violently while it was cloſing, ſo that there could not have been much air in the tube. This air, which muſt therefore have been generated in the tube, was a little worſe than common air, being of the ſtandard of 1.12 when the latter was 1.04. I repeated the experiment ſeveral times, and always with the ſame reſult.

That this air ſhould be worſe than common air, I cannot well explain. But in my former experiments it appeared that vitriolic acid air injures common air; and that in proportion as pure air is expelled from this acid, the remainder becomes phlogiſticated, or charged with vitriolic acid air, clearly appeared in the following experiment.

Making a quantity of oil of vitriol boil in a glaſs retort, and making the vapour paſs through a red-hot earthen tube, glazed inſide and out, and filled with pieces of broken tubes, I collected the liquor that diſtilled over, and found it to be the ſame thing with water impregnated with vitriolic acid air. The ſmell of it was exceedingly pungent, and it was evident, that more of this air had eſcaped than could be retained by that quantity of water. The oil of vitriol uſed in this proceſs was 1 oz. 9 dw. 18 gr. and the liquor collected was 6 dw. 12 gr. When I collected the air that was produced in this manner, which I did not do at this time, it appeared to be very pure, about the ſtandard of 0.3 with two equal meaſures of nitrous air.

At another time, expending 1 oz. 11 dw. 18 gr. of oil of vitriol, of the ſpecific gravity of 1856 (that of water being 1000), 1000), I collected 19 dw. 6 gr. of the volatile acid, of the ſpecific gravity of 1340, and 130 oz. meaſures of dephlogiſticated air of the pureſt kind, *viz.* of the ſtandard of 0.15.

It is eaſy in this manner to collect a great quantity of dephlogiſticated air; but the principal objection to the proceſs is, that after uſing a few times, the earthen tubes become tender, and too eaſily break, eſpecially in heating or cooling. It is alſo difficult to lute the retort containing the acid and the earthen tube. The air produced in this manner is filled with the denſeſt white cloud imaginable.

Going through the ſame proceſs with ſpirit of nitre, the reſult was in all reſpects ſimilar, but much more ſtriking, the production of both dephlogiſticated air and phlogiſticated acid vapour being prodigiouſly quicker, and more abundant. Expending 5 oz. 8 dw. 6 gr. of ſpirit of nitre, I collected 600 oz. meaſures of very pure dephlogiſticated air, being of the ſtandard of 0.2. I alſo collected 1 oz. 7 dw. 14 gr. of a greeniſh acid of nitre, which emitted copious red fumes. All the apparatus beyond the hot tube was filled with the denſeſt red vapour, and the water of the trough in which the air was received was ſo much impregnated with it, that the ſmell was very ſtrong; and it ſpontaneouſly yielded nitrous air ſeveral days, juſt as water does when impregnated with nitrous vapour. Perceiving the emiſſion of air from the water, after it had ſtood ſome time, I filled a jar containing 30 oz. meaſures with it, and without any heat it yielded two oz. meaſures of the ſtrongeſt nitrous air.

Taking the ſpecific gravity of the acid before and after this diſtillation, the former was to the latter as 1471 to 1182. When the weight of the air produced in this experiment, and that of the liquor diſtilled, is compared with that of the acid before diſtillation, it will appear, that there muſt have been a great loſs of acid vapour, which was either retained in the water of the trough, or eſcaped through it.

I do not ſee that theſe experiments can be explained, but on the ſuppoſition that the moſt dephlogiſticated oil of vitriol and ſpirit of nitre are, in a proper ſenſe, ſaturated with phlogiſton; and that when part of the acidifying principle is expelled in the form of the air, the remainder is ſuperſaturated with it.

To try whether the acid, thus ſuperſaturated with phlogiſton, was convertible into pure air by this proceſs, I heated the liquor collected after the diſtillation of the oil of vitriol, that is, water impregnated with vitriolic acid air, and made the vapour paſs through the hot tube, but no air came from it; and when collected a ſecond time, it was not at all different from what it had been before. The ſpecific gravity was alſo the ſame.

It is evident, however, though this proceſs does not ſhew it, that the volatile vitriolic acid contains the proper element of dephlogiſticated air; ſince by melting iron in vitriolic acid air, a quantity of fixed air (which is compoſed of inflammable and dephlogiſticated air) is produced. Melting iron in 9 oz. meaſures of vitriolic acid air, it was reduced to 0.3 oz. meaſures, and of this 0.17 oz. meaſures was fixed air. I repeated the experiment with the ſame reſult, and putting the reſiduums together found the air to be inflammable.

But the reſult was ſomething different when I ſent through the hot tube the liquor that I had collected in the proceſs with ſpirit of nitre. No air, however, was produced at the firſt, nothing appearing beſides a *red vapour* that was wholely abſorbed by water, or eſcaped through it into the atmoſphere; but towards the end of the proceſs I collected 10 oz. meaſures of dephlogiſticated air. The quantity of the liquor expended was about 2 oz. meaſures. It may, however, be preſumed, that this ſmall quantity of air came from ſome of the acid which eſcaped the action of the fire in the former proceſs. Indeed its coming at the laſt only may be conſidered as a proof of this, as all the more volatile acid, which came over firſt, yielded no air.

I ſubmitted a quantity of *ſpirit of ſalt* to both theſe proceſſes, *viz.* expoſing it to a boiling heat in glaſs tubes, hermetically ſealed, and making the vapour paſs through a red hot earthen tube, but no air was produced in either caſe. In the former caſe, the water ruſhed into, and completely filled, the tube, when it was opened under water; and in the other proceſs the liquor diſtilled was preciſely of the ſame ſpecific gravity, and, no doubt, in all other reſpects, the ſame as before diſtillation; but the acid that remained in the retort was of leſs ſpecific gravity, in conſequence of the acid vapour being expelled by the heat in the form of marine acid air, which appeared not to be affected by a red heat.

Though, in the proceſs with ſpirit of ſalt, the reſult be different from that of thoſe with oil of vitriol and ſpirit of nitre, yet there is an analogy among all theſe three acids in this reſpect, *viz.* that the marine and both the volatile acids of vitriol and nitre are made by impregnating water with the acid vapour, ſo that in its uſual ſtate it may be ſaid to be phlogiſticated as well as theſe.

It was evident that the water in the worm-tub was much more heated by the diſtillation of the ſpirit of ſalt than by that of the oil of vitriol, and eſpecially that of the ſpirit of nitre; ſo that much of the heat by which it had been raiſed in vapour muſt, in the latter caſe, have been *latent* in the air that was formed; whereas, in the other caſe, it was communicated to the water in the worm-tub.

In one of the proceſſes with boiling ſpirit of ſalt, in a glaſs tube, hermetically ſealed; I had the ſame white vapour dancing in the middle of the tube as in the experiment with the oil of vitriol; but this tube burſt, and I never had the ſame appearance again, though I repeated the experiment ſeveral times for the ſake of it.

The vapour of dephlogiſticated marine acid, which M. BERTHOLLET diſcovered, and with which water may be impregnated as with fixed air, being made to paſs through the hot earthen tube, became dephlogiſticated air as in the following experiment.

Having poured a quantity of ſpirit of ſalt upon ſome manganeſe in a glaſs retort, I heated it as in the preceding experiments with a proper apparatus both for receiving the diſtilled liquor, and the air. I found ſeven-tenths of the air was fixed air, and the remainder very pure dephlogiſticated. The quantity I could not meaſure on account of one of the junctures in the apparatus giving way; but I do not imagine that quite ſo much pure air could be got in this method as from the manganeſe itſelf in a direct proceſs. The liquor received in this diſtillation reſembled ſtrong ſpirit of ſalt in which manganeſe had been put.

This proceſs immediately ſucceeding that in which the glaſs tube, joining the earthen tube and worm-tub, was left full of black matter by the diſtillation of the alkaline liquor (which will be mentioned hereafter), the blackneſs preſently vaniſhed, and the tube became tranſparent as before. On this account, however, it is poſſible that I might receive leſs pure air than I ſhould otherwiſe have done.

Diſtilled vinegar ſubmitted to this proceſs yielded air two-thirds of which was fixed air, and the reſt inflammable: expending 2 oz. 19 dw. 0 gr. of the acid, I got 1 oz. 19 dw. 0 gr. of a liquor which had a more pungent ſmell than it had before diſtillation. It had alſo ſome black matter in it, and ſome of the ſame remained at the bottom of the retort when the liquor was evaporated to dryneſs. The air I received was 90 oz. meaſures.

Alkaline air is converted into inflammable air in this proceſs as well as by the electric ſpark, but by no means, I think, in ſo great a degree. I put 2 oz. 10 dw. 0 gr. of water pretty ſtrongly impregnated with alkaline air into the retort, and heating it, ſent the vapour through the hot tube; when I collected 2 oz. 3 dw. 0 gr. of liquor, which had a diſagreeable empyreumatic ſmell, as well as that of a volatile alkali, and it was quite opaque with a *black matter*, which ſubſided to the bottom of the veſſel. Alſo the tube through which the air and vapour had been conveyed was left quite black, as mentioned above. One of the junctures of the apparatus not having been air-tight, I did not collect all the air, but it came only at the beginning of the proceſs, and before the tube became black, or any liquor was diſtilled, and it was all ſtrongly inflammable.

I ſhall now recite a few experiments of a different kind from thoſe that have been mentioned above, and more immediately relating to the doctrine of phlogiſton.

It is ſaid, by thoſe who do not admit the doctrine of phlogiſton, that the metals are ſimple ſubſtances, which, having a ſtrong affinity to dephlogiſticated air, imbibe it when they become calces, without parting with any thing. But that ſomething is really parted with in the calcination (as they will call it) of iron in dephlogiſticated air, appears to me to be very evident, as well as in the proceſs with ſteam.

That fixed air is found in the veſſel in which iron is melted in dephlogiſticated air, I obſerved before; but I never took much care to aſcertain the quantity of it. This I have lately done in many inſtances, and in all of them find it to be much more conſiderable than can be accounted for, by ſuppoſing it to come from plumbago in the very ſmall quantity of iron that I melted; ſo that it muſt neceſſarily have been formed by the phlogiſton from the iron, and the pure air in the veſſel, at the ſame time that the iron became finery cinder by imbibing water from the air; and I have ſhewn, that by far the greateſt part of the weight of this air is water. The experiments were made with a very good burning lens, of ſixteen inches diameter, with which Mr. PARKER has generouſly furniſhed me; and by means of it I can now make theſe experiments, which require a great degree of heat, with much more eaſe and certainty than I could do before.

In 6½ oz. meaſures of dephlogiſticated air I melted turnings of malleable iron till there remained only 1⅓ oz. meaſure, and of this $\frac{27}{30}$ oz. meaſure was fixed air. In 6 oz. meaſures of dephlogiſticated air, of the ſtandard of 0.2, I melted iron till it was reduced to two-thirds of an ounce-meaſure, of which one-half was fixed air, and the remainder completely phlogiſticated. Again, I melted iron in 7½ oz. meaſures of dephlogiſticated air, of the ſame purity with that in the laſt experiment, when it was reduced to 1⅓ oz. meaſure, and of this four-fifths was fixed air, and the remainder phlogiſticated. In this caſe I carefully weighed the finery cinder that was formed in the proceſs, and found it to be nine grains, ſo that the iron that had been melted (being about two-thirds of this weight) had been about ſix grains. I repeated the experiment with the ſame reſult.

When the dephlogiſticated air is more impure, the quantity of fixed air will always be leſs in proportion. Thus, having melted iron in ſeven ounce meaſures of dephlogiſticated air of the ſtandard of 0.65, it was reduced to 1.6 oz. m.; and of this only one-third of an ounce meaſure was fixed air. This, however, is much more than can come from the plumbago in the iron; but as the production of this fixed air is by many aſcribed to this plumbago, it may be worth while to ſhew by computation that it is impoſſible that it ſhould have this origin. Both the quantity of plumbago in iron, and the quantity of fixed air in plumbago, are much too ſmall for the purpoſe.

From half an ounce of the pureſt plumbago, I firſt got, in a coated glaſs retort, 13 ounce-meaſures of air, of which only three ounce meaſures were fixed air, the reſt being inflammable; then putting it into an earthen tube, I kept it ſome hours in as great a heat as I could produce, and got 22 oz. m. more; and of this alſo only three were fixed, and the reſt inflammable, and the laſt portion was wholly ſo.

But inſtead of ſuppoſing the fixed air that I got to be that which was expelled from the plumbago in the iron, I will ſuppoſe that even the whole of this plumbago afforded only one of the elements of the fixed air, *viz.* phlogiſton, or that which the French chemiſts call *carbone*; and that this principle, by its union with the dephlogiſticated air in the veſſel, forms the fixed air, yet on this moſt unfavourable and improbable ſuppoſition the quantity will be found to be inſufficient.

If 100 gr. of iron contain, according to M. BERGMAN, 0.12 gr. of plumbago, 7 gr. (which is the moſt that in any of the preceding proceſſes I converted into finery cinder) would contain only 0.0084 gr. of plumbago; and if we ſuppoſe with Mr. KIRWAN, that an hundred cubic inches of fixed air contains 8.14 gr. of phlogiſton, the fixed air produced in one of the above-mentioned proceſſes (*viz.* four-fifths of an ounce-meaſure) would contain .032 gr. of phlogiſton, which is above three times more than the plumbago in the iron could furniſh. It is evident, therefore, that the quantity of fixed air that I found muſt have been formed by phlogiſton from the iron uniting with the dephlogiſticated air in the veſſel.

If, as I have inferred, from burning charcoal of copper in dephlogiſticated air (ſee Experiment, Vol. VI. p. 272.) fixed air conſiſts of 3.45 parts of dephlogiſticated air and 1.5 of phlogiſton, it will be found, that four-fifths of an ounce meaſure of fixed air will contain 0.21 gr. of phlogiſton, which is much more than on the ſuppoſition of Mr. KIRWAN.

Another argument againſt the antiphlogiſtic doctrine may be drawn from an experiment which I made upon Pruſſian blue; if the ſmall quantity of fixed air, that may be expelled from it by heat, be compared with the much greater quantity which is produced when heated in dephlogiſticated air.

Pruſſian blue is generally ſaid to be a calx of iron ſuperſaturated with phlogiſton, though of late it has been ſaid by ſome that it has acquired ſomething that is of the nature of an *acid*. From my experiments upon it, with a burning lens in dephlogiſticated air, I ſhould infer, that the former hypotheſis is true, except that the ſubſtance contains ſome fixed air, which is no doubt an acid; for much of the dephlogiſticated air diſappears, juſt as in the preceding ſimilar proceſs with iron.

I threw the focus of the burning lens upon 2 dw. 5 gr. of Pruſſian blue in a veſſel of dephlogiſticated air, of the ſtandard of 0.53, till all the colour was diſcharged. Being then weighed, it was 1 dw. 2 gr. In this proceſs 7¼ oz. of fixed air had been produced, and what remained of the air was of the ſtandard of 0.94. Heating the brown powder to which the Pruſſian blue was reduced in this experiment in inflammable air, it imbibed 8½ oz. m. of it, and became of a black colour; but it was neither attracted by the magnet, nor was it ſoluble in oil of vitriol and water, as I had expected it would have been.

Again, I heated Pruſſian blue in dephlogiſticated air, of the ſtandard of 0.2, without producing any ſenſible increaſe of its bulk, when I found three ounce meaſures of it to be fixed air, and the ſtandard of the reſiduum, with two meaſures of nitrous air, was 1.35. The ſubſtance had loſt eleven grains, the greateſt part of which was evidently water.

To determine what quantity of fixed air Pruſſian blue would yield by mere heat, I put half an ounce of it into an earthen tube, and got from it 56 oz. m. of air, of which 16 oz. m. were fixed air, in the proportion of one-third in the firſt portion, and one-fourth in the laſt. The remainder was inflammable. There remained 5 dw. 20 gr. of a black powder, with a very little of it (probably the ſurface) brown.

Comparing theſe experiments, it will appear, that the fixed air procured by means of Pruſſian blue and dephlogiſticated air muſt have been formed by phlogiſton from the Pruſſian blue and the dephlogiſticated air in the veſſel: for if 240 gr. of this ſubſtance yield 16 oz. meaſures of fixed air, ten grains of it (which is more than was uſed in the experiment) would have yielded only 0.6 oz. m. Nor is it poſſible to account for the diſappearing of ſo much dephlogiſticated air, but upon the ſuppoſition of its being employed in forming this fixed air.

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XXIV. *On the Production of nitrous Acid and nitrous Air, By the Rev.* Iſaac Milner, *B. D. F. R. S. and Preſident of* Queen's College, Cambridge.

Read July 2, 1789.

1. IT has been known for ſome time, that a relation ſubſiſts between nitrous acid and volatile alkali. The latter has frequently been produced by help of the former; but I do not recollect that, in any inſtance, the volatile alkali has been proved to contribute to the formation of nitrous acid or nitrous air. Some caſes, however, have occurred to me where this evidently happens; and they appear ſo new and extraordinary, that I cannot but think they deſerve the attention of philoſophical chemiſts. The hiſtory of the experiments I allude to is as follows.

2. As ſoon as I had heard of the production of inflammable air by the tranſmiſſion of ſteam through red-hot iron tubes, I had the curioſity to try whether ſome other ſubſtances in the form of air or vapour might not, by a ſimilar proceſs, undergo material alterations. In particular, the nitrous acid ſeemed well to deſerve a trial, both on account of the obſcurity and difficulties attending the theory of its production, and alſo of its important and extenſive uſefulneſs in chemiſtry.

In the relation of my experiments on this head, it will be unneceſſary to mention the exact *quantity* of acid or of air expended or generated, though I noted thoſe quantities pretty accurately at the time; for the main point I have in view in this deſcription, is to aſcertain the *nature* of the *changes* which took place; and theſe do not depend upon the quantities of aerial fluids, but upon their properties. Beſides, whoever ſhall repeat theſe experiments will find the relative *quantities* to vary very much, according to the manner of operating; and therefore, for the ſake of brevity, I omit to mention them entirely.

3. I began with boiling a little ſtrong nitrous acid in a ſmall retort, the neck of which was cloſely luted to one end of a gun-barrel. The other end of it was immerſed ſometimes in water, and ſometimes in quickſilver, and eighteen or twenty inches of the middle part was ſurrounded with burning charcoal in a proper furnace. In this manner the vapour and fumes of the boiling acid were tranſmitted through the red-hot tube, and the produce received at the end in the uſual manner.

When the acid was made to boil violently, there paſſed over a conſiderable quantity of undecompoſed red nitrous vapour, together with a mixture of nitrous and phlogiſticated airs.

When the proceſs was conducted more moderately, there was leſs nitrous vapour; and in the mixture of airs which was received in the glaſs veſſels, there was a much greater proportion of phlogiſticated air.

4. In order to increaſe the ſurface of the red-hot iron, and effect a more complete decompoſition of the nitrous vapour, the gun-barrel was crammed full of iron filings. The experiments were repeated with great caution, and almoſt the whole of the produce was found to be phlogiſticated air. It is however proper to mention, that, notwithſtanding every poſſible care, ſtill there will generally be in ſome degree an admixture

of nitrous air, and frequently of dephlogiſticated nitrous air. But I am ſatisfied that if the iron tube were ſufficiently long, ſo that a very large portion of it might be heated red-hot, all the air received in this manner from any quantity of nitrous acid ſlowly boiled would be found of that ſpecies called phlogiſticated air.

5. Theſe experiments ſeem altogether analogous to thoſe of Dr. PRIESTLEY, in which nitrous air, by expoſure to iron, is converted firſt into dephlogiſticated nitrous air, and afterwards into phlogiſticated air. The only difference ſeems to be, that in my experiments the effect is brought about ſuddenly; whereas in the method of expoſition to iron much time is required. And farther, in my method of operating, it is very difficult to conduct the proceſs ſo as to inſure the production of that ſingular ſpecies of air called dephlogiſticated nitrous air. If the acid boil very quick, the product is nearly all nitrous vapour and nitrous air. If it boil very ſlow, and a ſufficient quantity of the iron tube be well heated, then the decompoſition is almoſt complete, and little is received but phlogiſticated air. In both caſes, the progreſs of the converſion of nitrous acid to the ſtate of phlogiſticated air ſeems to be the ſame. Firſt, nitrous air is formed, then dephlogiſticated nitrous air, and laſtly phlogiſticated air. This, I ſay, ſeems to me to be the natural order of the converſion, though I do not deny, that in the rapid manner of operating with the red-hot iron tube ſome particles of nitrous acid or vapour may probably be *inſtantly* changed into phlogiſticated air. And even allowing this to be the caſe, the fact may eaſily be explained, by ſuppoſing the ſucceſſive approaches to phlogiſticated air to be made in too ſmall ſpaces of time to be obſerved; nor does it in the leaſt invalidate the general concluſion, that nitrous

air is nearer the ſtate of phlogiſticated air than nitrous acid or nitrous vapour; and that dephlogiſticated nitrous air is ſtill nearer. It is very difficult to decide with certainty what the changes are which the particles of the acid undergo in their paſſage through different parts of the hot tube.

From what has been ſaid, the moſt common proceſs will probably appear to be, that a particle of the acid in the form of vapour firſt generates nitrous air; that the parts of this are applied to freſh ſurfaces of hot iron, and ſuddenly changed into dephlogiſticated nitrous air; which, laſtly, is applied to ſtill freſh ſurfaces of the tube or fragments of iron, and ſo converted into phlogiſticated air. When theſe ſucceſſive contacts with freſh ſurfaces of hot iron are not ſufficiently numerous or exact, it is not unnatural to conclude, that ſome portion of air may eſcape not perfectly decompoſed.

6. Theſe conſiderations induced me to alter the proceſs a little. Inſtead of boiling the acid in the retort, I put ſome thin pieces of copper into a phial, poured nitrous acid upon them, and forced the nitrous air, as it was generated, to paſs through the red-hot tube. The event anſwered my expectation; the decompoſition was effected in this way eaſier than in the former.

But before I made this experiment, I examined what would be the effect of mere heat upon nitrous air, as I had already learned from the experiments of others, that nitrous acid, forced in the form of ſteam through red-hot tubes of clay or glaſs, underwent the moſt important alterations.

What might be the effect of long continued expoſure to a red heat I cannot ſay; but I was ſoon convinced, that nitrous air might be forced through a red-hot glaſs tube, without ſuffering any material change.

7. Laſtly, I determined to try the effect of the gun-barrel upon dephlogiſticated nitrous air, as from all that I had ſeen it ſeemed reaſonable to expect, that this ſpecies of air would be the eaſieſt reduced to the ſtate of phlogiſticated air. For this purpoſe, I diluted a ſaturated ſolution of copper in the nitrous acid, and put pieces of iron wire into it, and as the neck of the retort which contained the ſolution was luted to one end of the gun-barrel, the dephlogiſticated nitrous air was expoſed in its paſſage to the action of the red-hot tube, and alſo to the ſurfaces of the red-hot iron turnings which it contained. In this caſe, when the proceſs is conducted with proper care, all the air which is received at the other end of the tube will be found phlogiſticated.

8. When the air received at the end of the gun-barrel was in the laſt mentioned ſtate, *viz.* perfectly phlogiſticated, I have frequently obſerved a white fume iſſuing along with the air, and ſometimes aſcending through the water or mercury into the glaſs receivers. Upon examining this white fume, I ſoon perceived by the ſmell that it contained volatile alkali. I was much ſtruck with the obſervation, and immediately recollected Dr. Priestley's relation of a ſimilar production by expoſing nitrous air to pieces of iron.

9. Moſt of the experiments hitherto related were made in the ſummer of 1786; in general they agree with thoſe of Dr. Priestley; the changes and productions are much the ſame, and the only new circumſtance is, as was obſerved at art. 5. The ſame effects are brought about *inſtantly* by the action of red-hot iron, which require much time by the method of ſimple expoſure to cold iron.

For which reaſon, though it gave me much pleaſure at the time to ſee ſuch curious tranſmutations brought about in a few minutes, yet it ſcarcely appeared worth while to trouble the Royal Society with a detail of the experiments; and I only preſume to do it now, becauſe the conjectures which I then formed have been ſufficiently verified by future experiments.

The conjectures were as follow:

10. Almoſt immediately upon ſeeing the volatile alkali produced by means of nitrous acid and metals, I conceived the poſſibility of inverting the order of the proceſs, and of producing nitrous acid or nitrous air by the decompoſition of volatile alkali. I knew of no experiments wherein this had been done, or any thing like it; yet as volatile alkali was beyond all diſpute produced in the method juſt deſcribed, and as the iron turnings and inſide of the gun-barrel were left after the operation in a ſtate of calcination, it ſeemed not unnatural to ſuppoſe, that by forcing volatile alkali through the red-hot calces of ſome of the metals, nitrous acid or nitrous air might be produced. Some of my friends, to whom I mentioned the idea, conſidered it as a random conjecture. However, I made a memorandum of it as a thing that deſerved to be tried, though in fact I neglected for near two years actually to make the trial. It was ſome time in the month of March, 1788, that the calx of mangancſe on account of its very great infuſibility, and its yielding abundance of dephlogiſticated air, occurred to me as a very proper ſubſtance for the purpoſe. I immediately crammed a gun-barrel full of powdered manganeſe; and to one end of the tube I applied a ſmall retort, containing the cauſtic volatile alkali. As ſoon as the manganeſe was heated red-hot, a lighted candle was placed under the retort, and the vapour of the boiling volatile alkali forced through the gun-barrel. Symptoms of nitrous fumes and of nitrous air ſoon diſcovered themſelves,

and by a little perſeverance I was enabled to collect conſiderable quantities of air, which on trial proved highly nitrous. I have ſince frequently repeated this experiment, and have always in ſome degree ſucceeded. Much depends on the *kind* of manganeſe employed, much on the heat of the furnace, and much on the patience of the operator; as theſe are varied, there will be great variations of the products. A minute detail of all the particulars of my experiments ſeems unneceſſary; but it may be proper to give a general account of the principal facts, and of the methods which were uſed to avoid erroneous concluſions.

11. In general I made uſe of clean gun-barrels with which no previous experiments had been made. The manganeſe was uſed in rough powder; for when it is too finely powdered, the tube is choaked, and the air cannot paſs.

In ſome experiments I applied the vapour of the volatile alkali directly to the hot manganeſe. In others I ſuffered the manganeſe to remain a conſiderable time in a red heat before I made the volatile alkali, contained in the retort at the end of the tube, to boil; and by this means I informed myſelf of the nature of the airs which the manganeſe yielded *per ſe*.

In neither caſe could I ever perceive the leaſt appearance of nitrous acid or nitrous air till the volatile alkali was uſed. Manganeſe, *per ſe*, gives airs of different kinds (but chiefly fixed and dephlogiſticated airs) as ſoon as ever it is ſubjected to a conſiderable heat; but nothing nitrous comes from it, either on the firſt application of heat, or after it has been continued a long time; and I examined this point with great diligence. But ſoon after the volatile alkali begins to be applied, the jars in which the air is received will frequently turn ſlightly red, and this redneſs will increaſe on admitting atmoſpherical air.

The cauſtic alkali ſhould be ſtrong, and as far as I have obſerved the longer the proceſs is continued, the ſtronger will be the nitrous air produced. At leaſt this evidently appeared to be the caſe in ſeveral inſtances, where the operation was continued for a long time.

In moſt inſtances, on the very commencement of this proceſs, a ſmall jar of the air thus collected diſcovers by the *ſmell* a nitrous impregnation. But it ſometimes happens, that ſeveral jars of air may be collected, and the admiſſion of atmoſpherical air to them will not produce a ſenſibly red colour.

Here, however, there exiſts a cauſe of deception againſt which the operator ought to be on his guard, leſt he ſhould conclude that no nitrous air is formed, when in reality there is a conſiderable quantity. The volatile alkali, notwithſtanding every precaution, will frequently paſs over in great quantities undecompoſed. If the receivers are filled with water, a great part of this will indeed be preſently abſorbed; but ſtill ſome portions of it will mix with the nitrous air formed by the proceſs. Upon admitting the atmoſpherical air, the nitrous air is decompoſed, and the red nitrous fumes inſtantly combine with the volatile alkali. The receivers are preſently filled with white clouds of nitrous ammoniac; and in this manner a wrong concluſion may eaſily be drawn, from the want of the orange colour of the nitrous fumes. A conſiderable quantity of nitrous air may have been formed, and yet no orange colour appear, owing to this circumſtance; and therefore it is eaſy to underſtand how a ſmall quantity of nitrous air may be moſt effectually diſguiſed by the ſame cauſe.

12. Theſe obſervations are made principally for the ſake of thoſe who may wiſh to repeat theſe experiments. The main point to be eſtabliſhed, is the actual formation of nitrous air by this method. And this truth I conſider as proved beyond all controverſy; for by continuing the proceſs patiently, and applying repeatedly freſh portions of ſtrong volatile alkali to the ſame manganeſe, kept conſtantly hot in the gun-barrel, I have often collected large jars of air, which was proved to be highly nitrous by mixture with atmoſpherical or with dephlogiſticated air.

13. It is not eaſy to ſay, whether in this proceſs dephlogiſticated nitrous air, or even nitrous acid itſelf, be not ſometimes immediately formed by the action of the volatile alkali on the manganeſe. Traces of the former, in ſome inſtances, ſeem to diſcover themſelves; but I do not ſpeak decidedly on this head. As to the latter, it is very certain, that fumes of the nitrous acid often circulate in the jars that receive the air. But poſſibly theſe fumes may ariſe from the decompoſition of nitrous air, by means of the ſuperfluous dephlogiſticated air of the manganeſe.

14. The ſteam of boiling water was applied to red hot manganeſe in a ſimilar way; not the leaſt nitrous appearance; but the fixed and dephlogiſticated airs were generated much more plentifully than when the manganeſe was urged by mere heat. When theſe airs had been collected in large quantities, the volatile alkali was applied as before to the reſiduum of the manganeſe, and nitrous air ſoon appeared.

15. As manganeſe is known to produce a very extraordinary change upon ſpirit of ſalt in a moderate heat, it ſeemed not improbable, that a ſtill greater change might take place by working in this method. Accordingly I forced the vapour of

boiling ſpirit of ſalt to paſs through red-hot manganeſe. This experiment did not anſwer my expectation; the product was a mixture of fixed and inflammable air. But it deſerves to be noticed, that even in this caſe, after the effect of the ſpirit of ſalt had been tried for a long time, a production of nitrous air upon the application of volatile alkali to the ſame manganeſe ſoon took place.

16. As there are many other ſubſtances beſides the calx of manganeſe, which are known, *per ſe*, to afford dephlogiſticated air, or a mixture of this with fixed air, it was natural to conclude from analogy, that ſuch ſubſtances upon the application of volatile alkali would not fail to afford nitrous air.

It is beſt, however, in theſe matters to truſt as little as poſſible to conjectures, and to bring every opinion to the teſt of experiment. Manganeſe is ſo ſingular a ſubſtance, that it is perhaps hardly ſafe, from what happens in making trials with it, to infer in any inſtance of another calx of a metal a ſimilarity of effect. Red lead, however, is known to agree in ſuch a variety of chemical effects with manganeſe, that I find it difficult to perſuade myſelf that the volatile alkali properly applied to it would not yield nitrous acid or nitrous air; yet I have hitherto in vain attempted to bring this about. The red lead, indeed, melts during the proceſs, flows into the cooler parts of the tube, and often choaks the paſſage of the air; but in ſome trials a great deal of air has been collected before that happened, and without any ſymptom of a nitrous mixture. It ſeems difficult to explain the reaſon of the failure; perhaps with a better adapted apparatus, and more perſeverance, either the production in queſtion may be obtained, or the cauſe of the failure diſcovered.

17. With calcined green vitriol I had much better ſucceſs. The ſalt was calcined to whiteneſs, and put into a gun-barrel; and, after ſeveral trials of forcing the volatile alkali through the hot tube, I procured by the operation ſome ounces of ſtrong nitrous air.

So extraordinary an effect would no doubt have proved highly grateful to the ancient chemiſts, and have been by them denominated a tranſmutation.

In the courſe of my enquiries, I conſidered this experiment as important, becauſe it proved, that the ſame combinations might take place when ſubſtances were made uſe of different from manganeſe.

18. As calcined green vitriol, *per ſe*, in a ſtrong heat yields dephlogiſticated air, I had now no doubt but that any ſubſtance which had this property might, by ſimilar treatment, be made to afford nitrous air.

But in this ſuppoſition I was entirely miſtaken. The volatile alkali was applied to ſome calcined alum at the moment when it was yielding in a ſtrong heat plenty of dephlogiſticated air. The product was an aſtoniſhing quantity of inflammable air, mixed with hepatic air and actual ſulphur. The reſiduum of the alum had a ſtrong hepatic ſmell, and contained particles of perfectly formed ſulphur.

Moſt of theſe experiments, if not all, were repeated in earthen tubes inſtead of gun-barrels, and with the ſame ſucceſs.

19. It now only remains, that I ſhould briefly propoſe what occurs to me as the probable theory and explanation of the facts related.

The ingredients which enter into the compoſition of nitrous acid ſeem to be the two principles or elements of the atmoſphere,

ſphere, *viz.* phlogiſticated and dephlogiſticated air. That this is the caſe, there ſeems little reaſon to doubt. Both the compoſition and decompoſition of nitrous acid renders the ſuppoſition probable. For,

1. Nitrous air and dephlogiſticated air by mixture produce nitrous acid; and nitrous acid, by mere heat, is converted into a mixture of phlogiſticated and dephlogiſticated airs.

2. Nitrous air, by the methods already related, is changed into phlogiſticated air, and theſe methods ſeem to conſiſt in abſtracting from the nitrous air a quantity of dephlogiſticated air.

3. When nitrous acid and nitre are produced in a natural way, the proceſs is not well underſtood; but the preſence of the atmoſphere is known to be neceſſary.

4. Mr. Cavendish's experiment is deciſive on this point. The union of the two airs in queſtion is effected by means of the electrical ſpark, and nitrous acid is the product.

In the next place we are to conſider, that volatile alkali contains phlogiſticated air; for,

1. Volatile alkali, by mere heat, or by the electrical ſpark, is changed into a mixture of phlogiſticated and inflammable air; and,

2. The reſiduum of volatile alkaline air, after the calces of lead have been revived in it, is phlogiſticated air.

Therefore, when volatile alkali, in the form of fume or air, is applied to red-hot manganeſe, or calcined green vitriol (ſubſtances which are then yielding dephlogiſticated air), with theſe facts in view, it ſeems not difficult to conceive, that one of the ingredients of the alkali, *viz.* phlogiſticated air, ſhould combine with dephlogiſticated air, and form nitrous acid or nitrous air. If nitrous acid be formed, it will indeed in that heat, as has been obſerved, be inſtantly decompoſed; but if the effect of the union be nitrous air, that will ſuſtain the heat without decompoſition. How it happens, that nitrous air ſhould be formed, and not nitrous acid, or what the reaſon is, that nitrous air can ſuſtain a red heat without decompoſition, when nitrous acid cannot, I am unable to ſay; and it is better to acknowledge our ignorance than advance groundleſs conjectures. So much, I think, may be pronounced as certain, *viz.* that nitrous air contains leſs dephlogiſticated air than nitrous acid; becauſe it requires the addition of dephlogiſticated air to become nitrous acid.

And, laſtly, if I miſtake not, the experiment with the calcined alum proves, that, in order to produce nitrous air, it is not ſufficient merely to apply volatile alkaline air to a ſubſtance which is actually yielding dephlogiſticated air.

Perhaps the preſence of another ſubſtance is required, which has a ſtrong attraction for phlogiſton. Perhaps, in the experiments with the calces of manganeſe and of iron, the inflammable principle of the volatile alkali combines with the calces of the metals, and the phlogiſticated air, the other component part, unites with the dephlogiſticated air; and if ſo, it ſeems not improbable to ſuppoſe, that when alum is made uſe of, the inflammable principle of the volatile alkali having little or no attraction for clay, the baſis of the alum, ſhould combine with its acid and form ſulphur. If this reaſoning be true, then it follows, that the vitriolic acid has a ſtronger affinity to the inflammable principle than it has to phlogiſticated air; and the proceſs with the green vitriol and manganeſe is to be explained by the operation of a double affinity: the inflammable principle of the volatile alkali joins with the calx of iron, the baſis of the vitriol, or with the manganeſe, and the phlogiſticated air with the dephlogiſticated air produced by the acid in the red heat.

Thoſe who chuſe to reject the doctrine of phlogiſton muſt make the neceſſary alteration in theſe expreſſions; but the reaſoning will be much the ſame.

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